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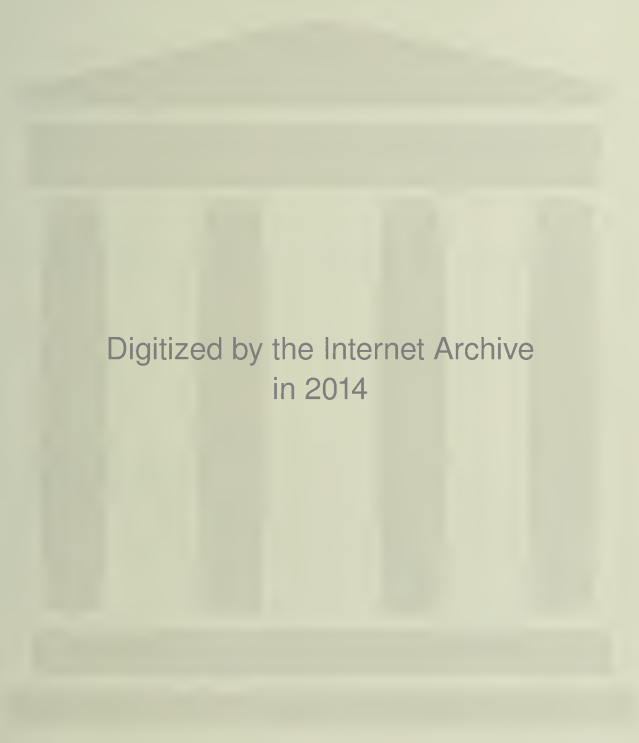
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THE ANNALS
OF
PHILOSOPHICAL DISCOVERY
AND
MONTHLY REPORTER
OF
THE PROGRESS OF PRACTICAL SCIENCE.

CONDUCTED
BY WILLIAM STURGEON,

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EXPERIMENTAL SCIENCE; LATE SUPERINTENDENT OF THE
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THE ANNALS
OF
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OF THE
PROGRESS OF SCIENCE AND ART.

NO. 1.

JULY, 1843.

Some Experiments and Remarks on the Changes which Bodies are capable of undergoing in Darkness, and on the Agent producing these Changes. By ROBERT HUNT, Esq., Secretary to the Royal Cornwall Polytechnic Society.

DR. DRAPER and M. MOSER do not appear to admit that the film of ioduret of silver on the Daguerreotype plates is decomposed by solar agency; the former attributing the change of colour to a disturbance produced by the absorption of the "tithonic rays," and the latter to a molecular change produced by light "externally blackening the iodide or other compound of silver."

My own impressions are certainly averse to this. I formerly expressed it as my opinion that the iodide was converted into an oxide of silver by the light. I now believe the oxidation to be a secondary action, due to the influence of the atmosphere. A great number of carefully conducted experiments have convinced me that the solar rays liberate iodine, leaving the silver in a state of very fine division loosely on the surface of the solid metal. Iodide of silver is insoluble in nitric acid or ammonia, although I find the former has the power of decomposing that salt by long-continued action; but it will be found that the darkened portions of the solarized Daguerreotype plates are rapidly dissolved by diluted nitric acid, and slowly by ammonia. If, as I have described in the paper already referred to, we receive an impression of the solar spectrum on an iodized plate, we may, by gently rubbing, remove all the darkened portion, and thus distinctly mark the spaces of maximum and minimum action, proving that some kind of decomposition has taken place. If we take the precipitated iodide of silver, we shall be enabled to trace more distinctly all that occurs. It has been remarked, that this salt exists in two states; the one sensible, and the other insensible to light. I find that perfectly pure iodide of silver undergoes little or no change by long exposure, but the presence of the smallest portion of nitrate of silver renders it very sensitive to luminous agency. If we take this sensitive iodide and allow it to blacken, and then throw it into diluted nitric acid, all the darkened

Annals of Philosophical Discovery, &c., No. 1, July, 1843. B

portion will be dissolved, and a pure non-sensitive iodide left behind ; or if we throw it into ammonia the darkened part will disappear, leaving the yellow salt, and in a few hours the ammonia will be covered with a pellicle of Faraday's oxide of silver.

According to Dr. Draper, if a properly prepared plate is exposed to light until in a fit state to receive the mercurial vapour and then placed in a dark room for four or five hours with a blackened *metallic* plate suspended one-eighth of an inch from its surface, we find, when it is exposed to the vapour of mercury, that those parts only are attacked by it which correspond with the suspended screen. It is inferred from this, "that the tithonicity that had originally disturbed the surface of the plate equally all over has escaped away from those portions that were uncovered ; but that its escape has been entirely prevented by the action of the screen, and this must be through radiation.... And further, that the rays that do thus escape away are absolutely invisible to the eye." Dr. Draper then proceeds to point out the analogy between this case and one of a body cooling by radiation, concluding, "the two cases are absolutely alike. Tithonicity therefore radiates exactly after the manner of heat." When we can account for the effects produced by known causes, we are not justified in seeking for new ones. In my paper on Thermography, I have shown that any blackened body placed above an iodized or a simply polished plate of metal,—whether that plate has been exposed to light or not is of no consequence,—will produce such a change as will dispose the covered portion to receive the vapour of mercury. If I can succeed in proving that all the phenomena described by Dr. Draper are to be produced as well on simple metals as on surfaces of the ioduret of silver or other sensitive surfaces, and equally in perfect darkness as in the brightest light, and that many of them are to be traced to chemical change, I shall certainly convince you of the impropriety of attributing them to the chemical, or, as Dr. Draper terms them, the tithonic rays.

As it was difficult, from the length of time required, to produce any decided effects upon the surfaces of polished metal plates by the solar spectrum without a good heliostat to keep the sun's image stationary for some hours, I was obliged to have recourse to different absorptive media in my endeavours to ascertain whether any of the rays of LIGHT were active in producing the effects described by Dr. Draper, Moser, and myself. I however obtained the most decided evidence that no effect was produced by an exposure of two hours to the prismatic spectrum by the most refrangible rays ; but it frequently happened in these trials, that much mercurial vapour was deposited about that part of the metal on which the rays of least refrangibility fell. These experiments were, however, by no means satisfactory ; but I hope, with the increase of light and the required instruments, to examine the question minutely this summer.

The coloured media used by me were,—1, red ; 2, yellow ; 3, green ; 4, blue : these insulated respectively the following rays :—

1. Red, orange, yellow, and some of the blue.
2. Orange, yellow, green, with a faint trace of the blue, and a small portion of the red.
3. Orange, yellow, green, and blue.
4. Green, blue, indigo, and violet.

In addition to these fluids, water (5), and water blackened with ink (6), were also used.

A very highly polished plate of copper with the above media in flint-glass flat bottles laid upon it, was exposed to bright sunshine for one hour. It was then placed in a dark box, and subjected to the vapour of mercury. The largest deposit of mercury was found to mark the space occupied by the red fluid (1); the next in order was that influenced by the blue (4), and then the others as follows: green (3), yellow (2), white (5), and black (6). There was less mercury deposited where the last four bottles of fluids had lain than on the uncovered portions of the plate. Precisely the same arrangement as the above was *kept in the dark* for five hours, and the plate then exposed to mercurial vapour, which attacked it in the same manner as when subjected to strong sunshine, except that no mercury was deposited on the uncovered parts of it.

Another copper plate was placed one-eighth of an inch above the same bottles of fluids in the dark, and allowed to remain in that position for five hours. On exposure to the vapour of mercury, it was found that the only impressions were made on those parts opposite the red fluid (1), the blue (4), and the green (3), the largest deposits corresponding with the red fluid; but that portion opposed to the green fluid (3) exhibiting the slightest possible traces of action.

I cannot explain any of the peculiarities which I have now described; they belong to a class of phenomena of the most mysterious kind, and the elucidation will only be effected by the most unwearying perseverance in experimental observation, and by a mind capable of drawing the most logical deductions therefrom. I state them now as they have occurred, for the purpose of showing how perfectly independent these effects appear to be of the chemical rays of the light. I may mention another very curious result in connection with the above. Two copper-plate engravings were placed upon highly polished amalgamated plates of copper; one of them was covered with common window-glass, and the other with a deep red glass stained with the oxide of gold. They were exposed to daylight for four hours, during which there were but faint gleams of sunshine. On subjecting the plates to the vapour of mercury, a very capital copy of the print was found to have been made under the influence of the red glass, but no trace of an impression under the other. It will be seen, on referring to Dr. Draper's paper, that these results correspond mainly with some obtained by him: but they will not admit of the explanation he has given of the radiation of tithonicity, unless he can in the first place prove the constant presence of the agent he has so named in all bodies. I know he at-

tempts to do this, but I am by no means satisfied that either Moser or Draper have as yet proved either the existence of "invisible light," or "dark tithonic rays."

With the view of testing Dr. Draper's results, I carefully iodized two silver plates and exposed them to light. I then placed them so that half of one plate was covered by half of the other, and allowed them to remain in the dark $\frac{1}{24}$ th of an inch apart for four hours. On mecurialization I could not detect the slightest difference between the covered and the uncovered portions of either of the plates.

Another silver plate was iodized and exposed to light. It was then placed in the dark with a sensitive plate which had been carefully kept from the light $\frac{1}{6}$ th of an inch above it, and a small engraving placed between them. They were allowed to remain thus for six hours. When exposed to the vapour of mercury, the plate which had been subjected to the light whitened all over, and the space occupied by the engraving was distinctly marked by lines of vapour thicker than the other parts. The plate which had been preserved in the dark was scarcely at all influenced by the vapour, except on those parts which had been touched by the supports of cardboard on which it rested. These were so arranged that no radiation could have influenced those parts of the plates.

An iodized silver plate was placed in the dark with a little fine string coiled over parts of it, and a polished silver plate supported $\frac{1}{8}$ th of an inch above it. After four hours both plates were subjected to mercurial vapour. On the iodized plate the deposit of vapour was uniform, although slight; but on the superposed plate of silver a strong and beautiful image of the string on the under plate became visible. I found that neither of the two iodized plates had lost their sensitiveness by the operations to which they had been subjected in the dark.

Hoping to detect some evidence of the process by which these singular results were produced, I instituted a series of experiments, of which the following are some of the most interesting results:—

a. A silver plate was iodized, a piece of card was placed upon it, and a well-polished mercurial plate (amalgamated copper) was suspended $\frac{1}{8}$ th of an inch above it, and left in this state for a night. The space on the silver plate corresponding with the mercurial plate, except under the card, was nearly freed of its iodine, which had evidently combined with the mercury on the upper plate. On exposing the mercurial plate to the vapour of mercury the image of the card was rendered visible, the vapour covering every part of the plate except that opposite the card. The silver plate received the vapour only on those parts which were not influenced by the mercurial plate. The upper plate was suspended by strings; these were faithfully imaged on both plates; by a thick line of mercurial vapour on the under plate, by the absence of it in the upper one.

b. An iodized *silvered* plate was exposed to light until brown, and a mercurial plate suspended above it for twelve hours. The browned

silver plate was *whitened*, and all the irregularities of the mercurial plate strikingly marked on it: the mercurial plate was slightly tarnished. On rubbing the silvered plate it was found that the silver was removed most readily over the whitened portion, but had lost none of its adhesion in other parts.

c. Over an iodized silver plate plates of gold, platina, silver, brass, copper, copper amalgamated, and zinc were placed, at the distance of $\frac{1}{8}$ th of an inch. After three hours the amalgamated plate had made a decided visible impression on the silver one. On exposure to vapour, the mercury lodged on every part of the plate but that affected by the mercurial plate; some irregularities were observed, but none which could be decidedly traced to the other metals in juxtaposition. I have some evidence that different metals near each other seriously interfere with each other's influence.

d. A mercurial plate was iodized, and another mercurial plate placed $\frac{1}{8}$ th of an inch above it. The upper plate became covered with a bright yellow film; and on exposing them to mercurial vapour, marks became apparent which corresponded with those in the opposite plate.

e. A silver plate was iodized and placed in the dark with an engraving, face down, upon it. An amalgamated copper plate was laid on this, and left for fifteen hours. The mercurial plate was reddened, and on exposure to the vapour of mercury, a very nice impression of the engraving was brought out, it having been effected through the thickness of the paper. On the silvered plate the space covered by the paper was well marked; but vaporization produced no trace of the engraving. The space beyond the paper was rendered white. It was curious that both plates had several spots which corresponded, particularly two, distinguished by a well-defined circle and a comet-like appendage, in length ten times the diameter of the circle. These spots could not be traced to anything visible in the print or either of the plates, and must, I think, be referred to some electrical influence. I find it indeed commonly the case, that the plates, after being subjected to these kinds of experiments a few times, become mottled, or present on their polished faces all the appearances of a finely-grained wood, and in this state they are less susceptible of receiving any impression than when not so.

f. A silver plate was iodized and placed upon an engraving laid on a brightly polished mercurial plate, and left in the dark for twenty-four hours. The mercurial plate was turned brown, and the silver plate was left in the same state as if it had been exposed to sunshine, being *brown and black*. Neither of these plates gave a copy of the picture.

g. A mercurial plate was iodized, and above it was placed a plate of polished iron, a disc of paper being first laid on the mercurial plate, and they were left in this state for some hours. On exposing the iron plate to mercurial vapour, it was abundantly lodged over that space opposite the paper disc, but not at all on the other parts.

The mercurial plate was attacked by vapour over every part but that which the paper disc protected.

Lead and zinc plates were used instead of the iron one with nearly similar results.

h. A Daguerreotype view was taken, and without removing the iodine a mercurial plate was placed a little above it, and left for ten hours. When removed, well-defined traces of the Daguerreotype picture were evident on the mercurial plate, which leads me to hope that by careful manipulation we may succeed in multiplying these beautiful productions by an easy method.

I became desirous of ascertaining whether the mercurial plates would produce any change upon the precipitated iodide of silver. I find by many experiments, that if the iodide of silver is pure, no more change is produced than is produced upon it by diffused light; but if it is rendered sensitive by a trace of the nitrate of silver, it is then darkened as by solar influence.

Sensitive iodide of silver being placed upon a plate of glass, a mercurial plate was fixed $\frac{1}{8}$ th of an inch above it. In three days the iodide of silver had become a deep brown, almost a black, and the mercurial plate was covered with the yellow iodide of mercury. Nitric acid dissolved the dark portion of the silver salt, as did also ammonia, on which was formed Faraday's oxide of silver, thereby proving the change, either by a primary or a secondary process, of the iodide into the oxide of silver. This experiment has been repeated at least a dozen times, and always with the same results. If a little heap of the iodide of silver is placed under a mercurial plate, it is exceedingly interesting to witness the gradual formation of the very beautiful coloured rings on the mercury in the progress of its conversion into an iodide. By prolonged action the yellow iodide passes into the bright red biniodide of mercury. I have some experiments now in hand, which convince me that similar chemical changes are to be effected through considerable spaces. I have succeeded in decomposing the iodide of copper and the iodide of gold by mercurial plates placed nearly a quarter of an inch above them.

I have an extensive record of results similar to those I have now detailed, all of them showing that the changes brought about by this mysterious agent, whether it be heat, light, or an undiscovered element, cannot be referred to those rays which the admirable researches of Sir John Herschel have shown to be the operative ones in producing the photographic phenomena which have so interested the world by their novel beauty, and which Professor Draper includes within his general term—tithonicity. With regard to the detithonizing influence of the gases mentioned by Dr. Draper in his paper in your March number*, I can only consider the results, which I find to be as he has stated, as the simple reconversion of the decomposed iodide of silver into another definite chemical compound. An iodized plate is exposed to light, the iodide of silver or

* See also *Annals of Electricity*, vol. x, p. 462.

other sensitive salt is decomposed, and in a state to receive mercurial vapour. It is now passed through an atmosphere of iodine, of chlorine, of bromine, or of nitrous gas. Chemists are well aware of the surprising energy with which these bodies attack the metals, consequently the exposure of a moment is quite sufficient to convert the surface which has undergone a change, into an iodide, chloride, bromide, or nitrate of silver. I certainly cannot see the necessity of going so far out of our way for an explanation of this effect as Dr. Draper has done.—[*Phil. Mag.*]

Tithonotype, or the Art of Multiplying Daguerreotypes. By J. W. DRAPER, M.D.

DR. DRAPER states at the commencement, that “the Daguerreotype image, in all its forms, may be transferred, by any copying process, to other suitable surfaces. In other words, it may be printed upon.” He considers that the Daguerreotype images are made up of a series of inequalities on the surface of the metal, and that they are to be ranged amongst “grooved, striated, or dotted surfaces.” Be this at it may, the casts which he procures from them, by the following process, give very favourable evidences of the correctness of his views.

“The Daguerreotype intended to be copied, is to be covered with a thin film of gold in the usual way, care being taken that the film is neither too thick nor too thin. If it be too thick the resulting copy is injured, and difficulties are more liable to arise in effecting the separation of the gelatinous coat; if too thin, the plate itself will suffer injury by having the figure torn off. A clear solution of isinglass is next to be prepared, and of such a consistency that a drop of it placed on a cold metallic plate will speedily solidify. This is to be particularly attended to, as much of the success of the operation depends on this solution being properly prepared. Dr. Draper has found that Cooper’s isinglass answers better than any other for this purpose.

“The Daguerreotype is to be supported with its face upwards in a current of hot air rising from a stove; and whilst thus situated, the isinglass is to be poured on it until a stratum of about one-sixth of an inch has accumulated. It is then suffered to dry: the whole process occupying two or three hours. When perfectly successful, the film of isinglass, now indurated into a *tithonotype*, peels off, and on being examined either by reflected or transmitted light, will be found to bear a minute copy of the original.

“Since the optical effect does not depend upon the thickness of the film, but on *surface configuration* alone, it appears to be very different from the chemical effect by which the phenomenon is produced.” From this fact the Doctor infers that, “the *Daguerreotype film*, which has been under the influence of light, is *polarized throughout its structure*, previous to mercurialization.” This polar-

ization is assimilated to the electro-polarization of a film of water which lies between the two platinum terminals of a galvanic battery, when decomposition of the water is going on. "As that water is polarized, and undergoes polar decomposition, so too do the same phenomena hold in the case of the Daguerreotype film."

From some previous experiments, Dr. Draper had ascertained that iodine is never separated from the plate "even under the most prolonged action of light;" and had supposed, that the "final appearance of the image is due to silver being liberated on the anterior face of the plate." Having also discovered that when the iodine and mercury are both removed from the plate, that the latter was corroded wherever the light fell, he concludes, therefore, that iodine "has been evolved on the posterior face of the film, and was the cause of the corrosion." It is in consequence of the iodine being evolved at the *back* of the film, and the silver at its *front*, that gives rise to the idea of the process being similar to that in the electro-decomposition of water. "The electro-positive and electro-negative elements" says our author, "are yielded up on opposite faces of the film, and its interior undergoes incessant polar changes,—the oppositely electric particles sliding, as it were, on one another."—[*Phil. Mag.*, May, 1843.]

Remarks and Observations on the Formation of Fogs. By Prof. W. M. CARPENTER, M.D.

METEOROLOGISTS, in examining into the causes which give rise to the formation of visible vapour, seem generally to have confined themselves almost exclusively to the theory of Hutton, considering it as incident upon the mixture of masses of air of different temperatures, and to have neglected in a very great measure the consideration of other causes. The intention of the following remarks will be to render it probable that other influences may, under favourable conditions, give rise to the same phenomenon; and also to show that the prevailing opinions respecting the conditions requisite to its production, are in some measure erroneous.

In the year 1819, a paper was published by Sir H. Davy, in the Philosophical Transactions, giving his views, and the result of his observations on the formation of fogs and mists. The following is a summary of the results at which he arrived. "After sunset, the earth commences to cool in consequence of radiation, but land and water are cooled by this operation at very different rates: the surface of the land cools much more rapidly than that of the water, and the air over the land becomes colder than that over the water, and when they both contain their due proportion of aqueous vapour, and the land is so situated as to permit the cold air from the land to mix with the warmer air over the water, the production of mist or fog will result. The density of such fog will moreover be greater as the land surrounding the water is higher, and the water deeper and warmer."

Here Davy follows Hutton, in attributing the formation of fog to the mixture of masses of air of different temperatures, and all the forms of visible vapour are now supposed to have their origin in some modification of the same influences.

He goes on to say, that in no case that came under his observation, "was fog formed on a river or lake, when the temperature of the water was lower than that of the air over it, even though the air was saturated with vapour." These remarks have been generalized and a law established, which I believe is generally received as true, that "*the formation of fog never takes place over water*, when its temperature is lower than that of the atmosphere, not even though the air should be saturated with vapour." Now we cannot hesitate about considering the observations of Davy as perfectly accurate, but it will be shown that the law is not of universal application, and that we should, consequently, protest against the too hasty generalization.

From a comparison of my own conclusions, drawn from observations on the fogs of our southern rivers, with those of Davy, drawn from observation on many of the great rivers of Europe, I should infer, either that the agencies governing their formation there must be different from those in which they originate here; or that his observations, particularly on rivers running towards the south, must have been mainly limited to the last of summer, the autumn, or first part of the winter.* For, were the conditions always observed by him to accompany the formation of fogs absolutely requisite for their production, it would preclude the possibility of their occurrence on the Mississippi River during a considerable portion of the year, particularly during the latter part of the winter, the spring, and first part of summer. For, the large body of water which comes down that river at those seasons, from much colder regions, traverses the climates through which it passes so rapidly, that it does not acquire their temperature; and when it arrives here its temperature is always much below the mean of those seasons, and even below the ordinary monthly minima. Convinced that this must be the case, and aware that the water of the Mississippi River is pleasantly cold for drinking during all the spring and first summer months, which required, at those seasons, that it should be considerably colder than the air, I

* Since the above was written, this inference has been confirmed by reference to Paris's Life of Davy. I give the names of rivers, on the fogs of which he made observations, with the place of observation, the general course of the rivers as to latitude, and the date of observation. 1st. The Rhine, between Cologne and Coblenz; course, north; date 31st of May. 2nd. The Danube, between Ratisbon and Vienna; general course of the waters from the sources, the tributaries being taken into account, northwardly; date, 9th, 10th, and 11th of June. 3rd. The Raab, near Kermond in Hungary; course, north; date, 11th July. 4th. The Save in Carniola; course, south; date, "end of August." 5th. The Ironzo, in the Friul; course, south; date, "middle of September." 6th. The Po, near Ferrara; course, south; date, "end of September." 7th. The Tiber; course, south; date, "beginning of October." So that only the Ironzo, the Po, and the Tiber, have southern courses to the points of observation, and on all these the observations were made during the autumn.

was satisfied that fogs, which are of common occurrence at those seasons, must be formed much more frequently over this body of water, when the temperature of the air was lower than that of the water. In the latter part of the year 1839, I commenced a series of observations with the view of testing the truth of these conclusions. The following table exhibits the results of observations, made during the formation or prevalence of fogs, when the temperature of the water was lower than that of the air.

Date of Observations.	Temperature of the water of the Mississippi River.	Temperature of the air at the surface of the water.	Temperature of the air at the height of six feet above the surface of the water.	Temperature of the air on shore.	Time of day at which the fog formed.
1842, Jan'y 11,....	46°	53°			10 A.M.
1841, Feb'y 15,....	49°	59°			10 A.M.
" " 25,....	50°	57°			11 A.M.
1840, March 27,....	53°	61·25	61·75	62·25	3 P.M.
" " 29,....	53°	60°	60·50	61°	11 A.M.
1841, April 1,....	56·50	61·50			Night.
" " 2,....	56·50	64°			3 P.M.
" " 5,....	56·50	63·50	63·75	64·25	Night.
" " 19,....	57°	71°			10 A.M.
1842, " 13,....	54°	66°			11 A.M.
" " 27,....	55°	59·50			9 A.M.
" " 28,....	55°	63°			Night.
" May 3,....	61·50	67°			Night.
" " 5,....	61·50	70°			11 A.M.
" " 13,....	61·75	67·50			Night.
" " 16,....	61·75	74°			2 P.M.
1841, June 19,....	69°	76°			10 A.M.

In one of the cases referred to in this table, the opportunities for making the observations were so favourable, the results so satisfactory, and the nature and variety of the phenomena so characteristic of the particular kind of fogs, that I will give a detailed account of the observations.

On the 27th of March, 1840, I was at the house of a friend, who lives on the banks of the Mississippi River. The weather was cloudy and rather cool for the season in this climate, the thermometer ranging from 55° to 65° Fahr., though it had ranged during some time previous at from 65° to 75°. The house was situated on the eastern bank, at the distance of about four hundred yards from the water's edge, the greater part of the distance intervening being a level plain, elevated at the time of observation only a few feet above the level of

the water, then at a high stage; but near the house the land rises abruptly, forming a bluff about fifty or sixty feet in height, and on this bluff, near the declivity, the house is situated.

About noon there was a light shower of rain, and it remained cloudy during the remainder of the day. At 3 o'clock P. M., being on the gallery, which commanded a view of the river, I perceived that fine wisps of vapour were forming in detached spots over the surface of the water. After examining the thermometer, which hung in a good exposure, I hastened to the water's edge, noted the temperature there, and again over the surface of the river, at the distance of about two hundred yards from the shore, where the temperature of the water was also observed. The results are given in the fourth line of the table, for March 27th, 1840, except that in addition, the temperature at the house on the bluff was 64° , being higher than on the bank.

The fog commenced to form in almost immediate contact with the surface of the water; indeed, when first observed, it had much the appearance of a very fine fleece floating in spots on the water. It thickened very fast, however, and before the observations over the water were concluded, which required perhaps ten minutes, the river was veiled in an impenetrable cloud, which stood over its surface to the depth of about fifteen feet. Although the upper part of the fog was elevated considerably above the level of the banks, it remained stationary over the bed of the river, and did not flow over the adjoining plain at all. The upper surface of the fog was level, and though defined with considerable accuracy, it terminated above in a thin haze; and although we could see nothing of a steamboat which passed near us, when we were on the bank, we had a fine view of all the upper parts of it after we had mounted upon a pile of wood, which raised us above the thickest part of the fog. The fog remained stationary over the surface of the river about an hour, and was then swept off by a brisk breeze which sprang up from the northwest.

The observations given above indicate the absense of those conditions which have generally been considered as requisite to the production of the phenomena under consideration. The temperature of the air was higher than that of the water, and that of the air over land, both on the immediate bank and on the higher lands back, was more elevated than that over the surface of the water. The fog could not have resulted from the mixture of the air from over land with that over the water, as the former was lighter than the latter, and could not flow down to the same level with it; neither could it have been produced by colder air from regions above the surface of the river settling down and mixing with that near the surface; for in this case we would expect to see the vapour commence to appear at some distance above the surface of the water, instead of in the air in immediate contact with that surface. We can hardly fail to attribute the production of fog in this instance to the refrigeration of the air, in consequence of communicating its heat to the water, and notwith-

standing the low conducting power of atmospheric air, we must suppose the extension of refrigeration to depend mainly on the conducting power of the humid air. If the diminution of temperature was confined to the stratum of air in immediate contact with the aqueous surface, the deposition of water on that surface would be the only result; but as the chill becomes general through the air above the river, fog will be formed, and will increase in depth and density as the impression is augmented.

The general conclusions at which I have arrived in reference to the occurrence of fogs over the Mississippi River, are as follows:—

1. That fogs form over this river at all seasons.

2. That they occur frequently during the day-time as well as at night.

3. That those occurring during the day-time, though often very dense, are not so extensive as those occurring at night, and rarely extend over land at all.

4. Fogs originating in day-time are of much more frequent occurrence from February to May than at other seasons, though the nocturnal fogs of winter frequently do not disappear before 10 or 11 o'clock in the morning.

5. In a large majority of the observations made by me, during the prevalence of fogs on the Mississippi River, in autumn and winter, the water had a higher temperature than the air. These have been exclusively nocturnal fogs, which frequently extend over large regions of country.

6. In almost every instance in which I have observed the formation of nocturnal fogs over the Mississippi River, during the spring and earlier part of summer, the river water has had a lower temperature than the air. These vary in extent, without any apparent connection with the relative temperature of the water and air; and the air over the higher lands is by no means invariably colder than that over and near the surface of the water.

7. In every instance in which I have observed fogs to form over the Mississippi River, in day-time, during the spring and earlier summer months, the temperature of the water was lower than that of the air; and the air over the land warmer than that over the water. These rarely extend over land much, and are most frequently confined to the air over the bed of the river.

8. In all the diurnal and in most of the nocturnal fogs of the spring and first part of summer, observed by me, on the Mississippi River, the production of visible vapour commenced near the surface of the water, and increased in depth by additions above.

It appears therefore that the fogs of the Mississippi River may be considered under two heads:—1st. Those which originate when the water is warmer than the air. 2nd. Those which originate when the water is colder than the air. The first kind prevails mainly during the fall and earlier part of winter; the second kind are most common during the spring and early part of the summer. The first

unquestionably results from the condensation of aqueous vapour, which rises constantly from the warmer water into the air, in quantities more than sufficient to saturate it at its temperature. The second, it appears to me, must result from the condensation of aqueous vapour already in the air, in consequence of the extension of refrigeration, from the cold surface of the water, through the warm and humid air above.

Among fogs which form over land, likewise, a large number have their origin independent of the causes assigned by the theory of Hutton. During almost every portion of the year, the high temperature of the air and earth during the day-time, increases the quantity of watery vapour in the air, and the dew-point temperature rises towards the hottest part of the day, so that the complement of the dew-point, except in winter, is rarely more than 10° or 15° Fahr., and most frequently not more than 5° or 10° . The temperature of the air gradually declines until about sunset, while the temperature of the dew-point remains almost fixed; thus diminishing the complement. After sunset, however, the earth cools rapidly by radiation, and the air participates, though not to the full extent, in the diminution of temperature.* It is not unusual to see the temperature of the air diminished by 15° or 20° between 3 o'clock P. M. and midnight, and a thermometer, with the bulb blackened, exposed, would indicate a still greater reduction for that of the earth; while the complement of the dew-point of the preceeding day was not more than 5° or 10° . If the temperature of the earth alone suffered diminution, the effect would be limited to the deposition of dew, but at the same time that dew is being deposited upon the cooled surface, thus reducing the temperature of the dew-point, the temperature of the air is also diminishing, and it depends altogether upon the relative amounts of the two concurrent decrements, whether any of the atmospheric water will take the form of visible vapour in the air or not. My observations, though not sufficiently numerous to enable me to make a satisfactory application of mathematical formula to the results, will still enable us to come to some safe general conclusions on these points. When the diminution of atmospheric temperature and of the temperature of the dew-point depends on the influence of a cold surface, the former seems always to decline most rapidly. This may, I think, be accounted for if we observe upon what the rapidity of diminution in each case depends. The decrement of atmospheric temperature depends for its value—first, upon a certain amount of radiation to the cold surface; second, upon the conducting power of the air, that is, on its degree of humidity, or in other words, upon the temperature of the dew-point; third, upon the difference between the temperature of the air, and that of the refrigerating surface. The decrement of the dew-point temperature will depend—

* "In every calm, still night, the air nearest the earth is colder than that which is more distant from it, to the height of at least 220 feet, this being the greatest height to which M. Six's experiments relate."—*Wells, on Dew*, p. 95.

first, upon the deposition of dew, that is, it will be nearly as the amount of humidity; and second, upon the difference between the temperature of the dew-point and that of the cold surface.* If therefore we take c to represent the temperature of the surface, and σ and σ' to represent respectively the difference between the temperature of the surface and that of the air and dew-point, we shall have the temperature of the air $= c + \sigma$, and that of the dew-point $= c + \sigma'$; and if β be taken to represent the effect of radiation, we shall have the decrement of atmospheric temperature proportionate to $\sigma(c + \sigma') + \beta$; while the decrement of the dew-point temperature will be as $\sigma'(c + \sigma')$. Therefore the decrement of atmospheric temperature will be greater than that of the dew-point temperature, by $(c + \sigma')(\sigma - \sigma') + \beta$. If it be denied that radiation has any influence in this case, we still have the decrement of atmospheric temperature greater than that of the dew-point temperature, by $(c + \sigma')(\sigma - \sigma')$. Now this concurrent diminution, by unequal decrements, that of the atmospheric temperature being greatest tends to reduce this temperature to equality with that of the dew-point; and after the two temperatures have in this way become equal, any farther diminution in the same ratio must result in the production of visible vapour in the form of fog. This explanation, if correct, will apply equally to fogs which form over cold waters, and many which form over land. Over land they occur most frequently towards morning, and are preceded, and perhaps at first accompanied, by the deposition of dew. They are most common in autumn and spring. When they form over large fields, or over our extensive prairies, they first begin to appear as a thin haze near the ground, and become deeper by additions above; but in dense foreign regions the condensation first commences about the tops of the trees, which taken together constitute in fact, in the present instance, the radiating and refrigerating surface.

When heavy and extensive fogs are dissipated in the morning by the heat of the sun, the visible is converted into invisible vapour, and becomes a part of the atmospheric air; but this change is not usually permanent, for the mixed atmosphere is heated, and rising in consequence of diminished specific gravity, it soon arrives at a region where the temperature is as low as its dew-point, when it again takes the form of visible vapour. Clouds thus produced, very often succeed to heavy fogs in this country during spring and autumn; they are generally formed at very short distances above the earth. Often, indeed, the vapour does not lose the visible form at all, but rises slowly until it arrives at a small elevation, when it commences to move as an ordinary cloud. This horizontal motion commences, generally, when the vapour has risen to the height of from one hundred to three hundred feet, but this region of clouds

* These suppositions, though not strictly accurate, are sufficiently so for the present purpose, as the variations from the truth affect somewhat equally the two decrements.

attains a greater elevation as the day advances, until it reaches the ordinary height. This kind of cloud is generally carried over this part of the country, towards the north or northwest, and sometimes continues to pass over during the earlier part of the day, until 10 or 11 o'clock, A.M. They are not very dense, having rather a hazy appearance, but are very continuous; and it rarely rains from them while they retain this form.

These clouds, as well as the fogs from which they originate, exercise a highly beneficial influence on vegetation; the former protecting it from the direct heat of the sun, while yet moist from the dew and fog of the preceding night; and the latter, by putting a check upon nocturnal radiation, thus lessening the fluctuations of temperature.

Jackson, La., May, 9, 1842.—[*American Journal of Science and Arts.*]

Some Remarks on the methods in common use of obtaining the Mean Temperature of Places, and on the supposed difference between the Temperature of the Air and that of the Earth. By Prof. W. M. CARPENTER.

IT is stated by Humboldt and others, that the mean temperature of the coldest springs in warm climates is often lower than that of the air of the same places. If we examine those agencies in which atmospheric temperature originates, and by which terrestrial temperature is modified, we shall perceive that such a condition could not exist, and consequently, that the observations on which such conclusions were based, were not accurate, or that some unsuspected agency must modify the relations which should otherwise be constant. In examining the meteorological records of our own country and of other parts, and comparing the observations made by different persons resident at the same places, we shall perceive that the results differ not more from the mean temperature of the place, than from each other. These discrepancies, or rather these departures from accurate results, are dependent on many circumstances. In the first place, thermometers of very inferior quality are in very general use, and they will often differ in the results afforded by four or five degrees. In the next place, sufficient importance is not attached to the position of the instrument; and instead of having it completely protected from reflection, and in almost complete obscurity, taking care at the same time to secure a free circulation of air about it, we find them often in such situation as to receive from walls or the earth a considerable portion of reflected heat; often in closed apartments or against walls, the temperature of which is influenced throughout the day by the full force of the sun. Galleries fronting the north are favourite places for suspending thermometers, under the impression that no heat is reflected from that side. In this way we often find, in this climate, that when a thermometer properly placed gives a temperature of 90° or

91°, all the others in the vicinity will stand at 95°, 100°, or even 110°; so that the annual mean derived would be very greatly above the true one. There is, however, it appears to me, a source of error much more general: I speak of the methods of calculating the mean after the observations have been made, and of the adoption into general use of methods which have been found to give correct results at particular places. The methods most in use are the following, viz. to take three observations, and from these calculate the mean directly; some fix upon sunrise, 2 P.M. and sunset; others, as in the Army Meteorological Register, and in those of most of the meteorological societies, upon 7 A.M., 2 P.M. and 9 P.M. I have ascertained, by taking the mean of hourly observations made here by myself, that any of these methods give means which, during every season of the year, are too high, and that the error in excess is greater, in proportion as the diurnal exceeds the nocturnal temperature. The last method, which is now in most general use, has probably been adopted in colder climates, as agreeing experimentally in its results with those derived from more frequently repeated observations during the same period; or possibly under the impression, that the 9 P.M. observation would give an approximation to the nocturnal mean, while the diurnal temperature would result as a mean from those at 7 A.M. and 2 P.M. In either case this method might be the best adapted for many climates; but as the relative value of the temperatures at the times fixed for observation are not constant, but will vary in passing from one climate to another, there is nothing in the method to suit it for universal use. The fact is, it appears to me almost impossible to fix upon any method, except that of hourly observations, which will in every place afford even an approximation to the truth, or obtain harmonious results from elements the relations of which are so variable as those of the temperatures of particular hours.

The method which I have adopted, and had in use about three years, I have found experimentally to give results, for this climate, almost in precise accordance with those derived from calculations based upon hourly observations. I have constantly kept daily observations at 6 and 7 A.M., 12 M., 2 P.M., sunset, and 9 P.M., so that I have been able to give my observation any of the common tabular forms, and have always obtained the general results according to each of the methods, and the results have verified my previous conclusions by varying from each other as follows: the mean annual temperature of this place, by my method, is 64·24° Fahr.; as derived from the sunrise, 2 P.M. and sunset observations, 66·30°; and during the same period, the mean obtained from the 7 A.M., 2 P.M. and 9 P.M. observations is 65·62°. In this way I have compared results by each method with each other, and with those obtained from hourly observations. I have found that the following method gives for this place results more accurate than any other I have been able to devise. The mean temperature of the diurnal

portion of the twenty-four hours is derived as the mean of the highest and lowest temperatures of that portion, that is, of the sunrise observation and that of 2 P.M. The nocturnal mean will in like manner result as the mean of the highest and lowest temperatures of that portion, that is, of the sunset and sunrise observations. The average of these two means will give the mean for the twenty-four hours. By this method, the sunrise temperature being the lowest for the twenty four hours, will belong to both the diurnal and nocturnal portions, and will enter twice into the calculations, while the other two observations will represent the maxima of the portions of the twenty-four hours to which they respectively belong.

If it is as I suspect, then, that the results of observations and calculations are, particularly in hot countries, frequently too high, a little attention to the subjects above hinted at may show that there is really no difference between the mean temperature of climates and of water derived from such depths as not to be affected by the change of seasons. In my tables the most exact agreement is shown between these temperatures at this place. The same appears to be the case in the island of Cuba, notwithstanding that we find the following statement in the table in which Prof. Kupffer compares the annual temperatures of places with that of the earth: "Havana, temperature of the earth, 74.30° ; of the air, 78.12° " Now Havana and Matanzas are in the same latitude, or nearly so, and there can be but little difference between the temperatures at the two places. Mr. A. Mallory, in this Journal, vol. xxxi, p. 289, gives the annual temperature at Matanzas as 77.06° Fahr., which he derives as a mean of the following averages of observations: sunrise 72.17° , 2 P.M. 81.41° , and sunset 77.61° Fahr. Now a mean of these observations would unquestionably give a mean too high by more than a degree, which would reduce the mean down to about 76° . My friend Mr. W. H. Potter has been kind enough to examine the temperature of wells, &c. for me, during a residence of a year in that island, particularly near Cardanus, in the same latitude and near Martanzas, and the observations frequently repeated through the year; and the temperature, apparently invariable, was 76° Fahr. thus affording a probability at least, that there also the mean temperature of the air, and of the earth at a certain depth, is the same.

What inferences might not be drawn by travellers in this country, particularly if they belonged to the anti-Huttonian school of geologists? I will not go out of my own neighbourhood. The temperature of water taken from strata whose average depth below the surface should be seventy or eighty feet, would be found to have at Baton Rouge a temperature of about 64.50° Fahr. But we are informed by the tables of the Army Meteorological Register, that the annual mean for that place is 68.07° , making a difference of 3.57° . Now we cannot mistrust the accuracy with which the calculations were made, but there can be no doubt that too high

a temperature has been obtained for want of attention to things generally considered as of minor importance, and in consequence of the too general adoption of methods which have been found to give correct results in other climates.

Jackson, La., May 9, 1842. — [*American Journal of Science and Arts.*]

A Review of the Researches upon the Dilatation of Gases by M. V. Regnault. By J. LAWRENCE SMITH, M.D.

On the dilatation of atmospheric air.—"There does not exist in physics a numerical element that has been submitted to so many experimental determinations, as the coefficient of the dilatation of air; and nevertheless we cannot say that this coefficient has been determined with sufficient precision.

"The beautiful experiments of Gay Lussac upon the dilatation of gases, seemed to have completely decided this question. He showed by a great number of experiments that the coefficient of dilatation between 0° and 100° cent. was the same for all gases and vapours, when they are not extremely near their point of condensation, and that its value was 0.375. This coefficient was adopted by all philosophers, and employed in their calculations, until M. Rudberg, a few years since, showed its inexactitude. By a series of experiments made with care, Rudberg sought to show that Gay Lussac's coefficient was too high, and that its true value was comprised between 0.364 and 0.365."

Regnault gives at length the experiments of Rudberg, and describes the instrument made use of; he then goes on to give a description of his own experiments, which were performed in four different methods, the first of which was similar to Rudberg's.

By the first method, the dilatation of air between 0° and 100° cent. is shown to be—

1	.	.	1.36556		8	.	.	1.36634
2	.	.	1.36626		9	.	.	1.36689
3	.	.	1.36659		10	.	.	1.36610
4	.	.	1.36579		11	.	.	1.36671
5	.	.	1.36625		12	.	.	1.36591
6	.	.	1.36549		13	.	.	1.36641
7	.	.	1.36673		14	.	.	1.36673

"The mean furnished by these fourteen experiments is 1.36623. The difference between the two extreme numbers is 0.00140, that is to say, a little more than $\frac{1}{7000}$ of the quantity that was measured. The numbers furnished by these fourteen experiments are all higher than the mean, 1.3646, that Rudberg obtained in his experiments, made by a similar process. I believe that this difference arises from the fact, that, in the experiments of Rudberg, aspiration of the external air took place; and it appears to me difficult to operate after his manner without this cause of error presenting itself, and it is evident that it must have escaped his attention, for he does not speak of it.

"The errors produced by this aspiration become more and more sensible, as the volume of air operated on is smaller. It was some time before I succeeded in avoiding this aspiration, and I am persuaded that it exercised a sensible influence upon my first experiments, making the numbers too small; and what confirms me in this opinion is, that from the moment that the aspiration became impossible, I never obtained a number lower than 1.3658."

In these experiments, it was necessary, in one step of the process, to immerse a glass tube, having a bulb at one extremity, into mercury, but the mercury not coming completely in contact with the tube, there would be a small portion of air adhering to it, and as the mercury would rise in the tube, the air would enter with it, and this is what Regnault here calls the *phenomenon of aspiration*; this he avoided by surrounding that part of the tube that was immersed in the mercury with tinfoil, and also by pouring a quantity of concentrated sulphuric acid upon the surface of the mercury.

Second method: the results obtained by this method are—

1	.	.	1.36629	10	.	.	1.36695
2	.	.	1.36645	11	.	.	1.36633
3	.	.	1.36593	12	.	.	1.36708
4	.	.	1.36610	13	.	.	1.56650
5	.	.	1.36585	14	.	.	1.36615
6	.	.	1.36590	15	.	.	1.36594
7	.	.	1.36615	16	.	.	1.36660
8	.	.	1.36591	17	.	.	1.36666
9	.	.	1.36708	18	.	.	1.36614

The mean of these experiments is 1.36633.

Third method: the results by this method are—

1	.	.	1.36688	7	.	.	1.36649
2	.	.	1.36688	8	.	.	1.36672
3	.	.	1.36612	9	.	.	1.36714
4	.	.	1.36643	10	.	.	1.36714
5	.	.	1.36651	11	.	.	1.36730
6	.	.	1.36626	12	.	.	1.36747

The mean of these experiments is 1.36679.

Fourth method.

1	.	.	1.36592	4	.	.	1.36682
2	.	.	1.36710	5	.	.	1.36674
3	.	.	1.36662	6	.	.	1.36580

The mean of these is 1.3665.

The following are the results of the four different methods:—

First Series	.	.	.	.	.	1.36623
Second Series	.	.	.	.	.	1.36633
Third Series	.	.	.	.	.	1.36679
Fourth Series	.	.	.	.	.	1.36650

Mean 1.3665

"Thus I adopt for the coefficient of the dilatation of dry air, for every degree of the Centigrade scale between the two fixed points of the thermometer, 0.003665."*

* This would be for every degree of Fahrenheit's scale 0.002036.

To this part there is a note, which is as follows: "M. Babinet has remarked to me, that in adopting for the coefficient of the dilatation of the air 0.366666... this coefficient is represented by the fraction $\frac{1}{36}$, which is very convenient to employ in calculation."

"The former coefficient admitted for the dilatation of air being found inexact, it is evident that we cannot regard as demonstrated, that it is the same for all gases, and new experiments are necessary to decide whether this law be rigorously true or only approximative.

"I have made experiments upon nitrogen, hydrogen, oxide of carbon, carbonic acid, sulphurous acid, cyanogen, protoxide of nitrogen, hydrochloric acid, and ammonia."

The following are the results of his experiments compared with air, dilatation between 0° and 100° cent.

For air,.....	mean of 50 experiments,	0.36650
„ nitrogen.....	3	0.36682
„ hydrogen	4	0.36678
„ oxide of carbon.....	2	0.36667
„ carbonic acid	4	0.36896
„ cyanogen	2	0.36821
„ protoxide of nitrogen .	3	0.36763
„ hydrochloric acid....	2	0.36812
„ sulphurous acid	3	0.36696

Regnault has just published a second series of experiments, and the coefficients for some of the gases have been changed ; the reason for it will be seen in what he says.

"In all the experiments that have been as yet described, the dilatation of the gases have been determined in an indirect manner. The increase of the elastic force of a certain volume of the gas under an elevation of temperature was measured, and from this the dilatation deduced, supposing Marriotte's law to be correct. But, one can object by saying that this law is not demonstrated to be absolutely exact even for air."

To leave no doubt as regards this mechanical theory of gas, I have made a new series of experiments, by means of a process that would enable me to measure directly the increase of the volume of the gas, (it being apparently under the same pressure at 0° and 100° cent.) This method is evidently the only one that we can employ for gases that do not follow the law of Marriotte under slight differences of pressure.

The instrument used was founded on the principle of M. Pouillet, *pyromètre à air*, and the results afforded by this series of experiments are—

For hydrogen	0.36613
„ atmospheric air	0.36706
„ oxide of carbon	0.36688
„ carbonic acid.....	0.37099
„ protoxide of nitrogen.....	0.37195
„ cyanogen	0.38767
„ sulphurous acid	0.39028

"The atmospheric air has afforded a number a little higher than

the mean of the preceding experiments ; but the difference is insensible, and besides it may be caused by the air not following rigorously the law of Marriotte.

"The oxide of carbon has given the same number as in the former experiments.

"The coefficients for carbonic acid and protoxide of nitrogen are much higher than those determined by the former experiments, and this is no doubt owing to the fact that these gases do not follow the law of Marriotte, and that their volumes at 100° cent., under the greater pressure to which it finds itself subjected at this temperature in the first experiments, are smaller than they ought to be according to this law."

As for the difference in the coefficients of cyanogen and sulphurous acid, Regnault has ascertained that it was owing to an error in the first experiments, the gases not being perfectly dry.

The dilatation of gases under different pressures, calculated after the change of the elastic force.—"Philosophers admit generally, that the dilatation of gases is constant between the same limit of temperature, whatever be the pressure to which they are subjected ; consequently, that it is entirely independent of the original density of the gas. But it is difficult to cite the experiments upon which this law is founded. Many observers having obtained the same value for the coefficient of the dilatation of air under different barometrical pressures, have concluded that it remains the same under all pressures. But the variations of the barometer in the same place are too small to permit us to come to so general a conclusion ; it serves to prove only that very small variations of pressure have no sensible effect in changing the coefficient of the dilatation of air.

The results of the experiments of Regnault upon air under different pressures are—

Pressure expressed in decimal fractions, the ordinary pressure being considered 1,00000.	Dilatation.
0.1444	0.36482
0.2294	0.36513
0.3501	0.36542
0.4930	0.36587
0.4937	0.36572
1.0000	0.36650
2.2084	0.36760
2.2270	0.36800
2.8213	0.36897
4.8100	0.37091

"The preceding experiments demonstrate that the law admitted by philosophers, that is to say, that the air dilates the same fraction of its volume whatsoever its density, is not exact. The dilatation of air between the same limits of temperature increases as the density of the gas becomes greater, or in other terms, as its molecules approach one another."

The same is shown to be true for carbonic acid.

Pressure expressed in decimal fractions, the ordinary pressure being considered 1,0000.	Dilatation.
1·0000	0·36856
1·1879	0·36943
2·2976	0·37523
4·7318	0·38598

Conclusions.—"My experiments do not confirm the two fundamental laws of the theory of gases, admitted up to the present time by all philosophers, viz.

"1. All gases dilate equally between the same limits of temperature.

"2. The dilatation of the same gas between the same limits of temperature is independent of its primitive density.

"Ought these laws now to be abolished? I do not think so. I believe that these laws, as well as all those that belong to the gases, such as the law of volumes, &c., are true to a *certain limit*; that is to say, that they approach so much the nearer to results of observation, as the gas is examined in a greater state of dilatation."

"The laws apply to a perfect gaseous state, to which the gases approach more or less, according to their chemical nature; according to the temperature at which we consider them, which can be more or less distant from each of those points where there is a change of condition; finally and above all, according as they are more or less compressed."—(*American Journal of Science and Arts.*)

A New Method of determining the quantity of Nitrogen in Organic Compounds. By Drs. VARRENTRAPP and WILL. Translated from the original in the *Annalen der Chemie und Pharmacie*. By J. LAWRENCE SMITH, M.D., of Charleston, S. C.

IN this letter I send you a translation of such parts of the original article of Drs. V. and W. as may enable one to thoroughly understand this valuable addition to what is already known upon the subject of organic analyses; neither the first nor the latter parts are here taken notice of, as the one is merely a detached account of the various processes that have been previously employed to estimate the quantity of nitrogen in organic compounds, and the other, the mention of some analyses made with the view of comparison.

"This method has for its basis the peculiar action of the hydrated fixed alkalis, upon organic substances containing nitrogen, when subjected to a high temperature. It consists in the separation of nitrogen in the form of ammonia, and estimating the latter, either under the form of the muriate of platinum and ammonia, or from metallic platinum."

"If one melts an organic substance free from nitrogen, together with the hydrate of potash, the water of the potash becomes decomposed, (as Gay Lussac has shown,) its oxygen combines with

the carbon and hydrogen of the organic matter, and its hydrogen passes off in the form of gas."

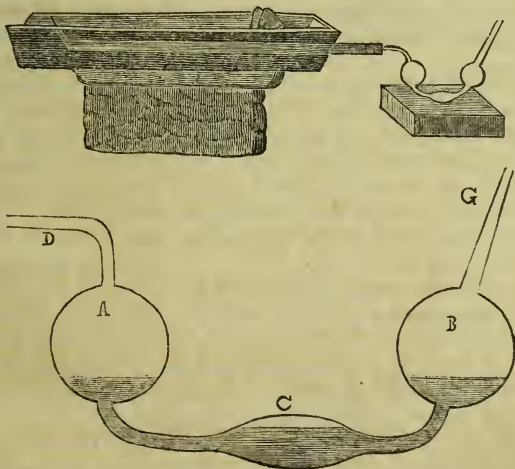
For a perfect result of the above nature a high temperature is required, as well as a considerable excess of potash.

"When on the contrary, the organic substance contains nitrogen, the free hydrogen combines with it and forms ammonia. Since the observation of this fact, the only use that it has been put to, has been that of ascertaining the presence of nitrogen in an organic compound."

The first and chief difficulty that presented itself to Drs. V. and W. in making use of this method to estimate the quantity of nitrogen, was that when a substance was very rich in that element, the whole of it would not be converted into ammonia, but that a portion by combining with the carbon formed cyanogen, which would pass off or be converted into hydrocyanic acid, and the latter unite itself to the potash. But upon experiment these gentlemen found, by employing a sufficient excess of hydrated alkali and a temperature not too low, that every cyanide and all other substances that contained nitrogen not under the form of nitric acid, would become decomposed by this means, and all the nitrogen be converted into ammonia.

"Our method, which is based upon the peculiar property already stated of all substances containing nitrogen, in which this element does not exist in combination, under the form of nitric acid, consists in the complete interception of the ammonia, by means of hydrochloric acid, and subsequently weighing it in a solid form as chloride of platinum and ammonia."

The apparatus used by Drs. V. and W. is such as is represented in the annexed figures. It consists of a furnace, such as is ordinarily used in organic analyses, with a tube of hard glass drawn out at its closed extremity. The length of this tube should be from twelve to



fifteen inches, and its inner diameter about three lines. To the open end of this tube is attached an apparatus, somewhat similar to Liebig's alkali bulbs, but differently constructed, to facilitate the pouring out of the liquid, which is placed within it. It is composed of three bulbs *a, b, c*, the two first being about one and a quarter inches in diameter, and the latter about five lines. The tube connecting these bulbs is about a line in diameter, and drawn out at its extremity *g* by means of a spirit lamp. The bulbs are filled to about the height represented in the figure, with *pure* hydrochloric acid of the ordinary strength, and this part of the operation is done by applying the mouth to the extremity *d*, and drawing the acid through the extremity *g*.

"As means of decomposing the organic substance containing nitrogen, that is to say, of oxydizing its carbon and hydrogen, we make use of the hydrate of potash, or soda mixed with caustic lime, in such proportions, that the mixture when exposed to a strong heat will not melt, but only slightly run together. This mixture has the advantage of being easily pulverized, and of not attracting moisture very rapidly, and generally it is managed with the same facility as the bioxide of copper or the chromate of lead. We have preferred latterly the mixture containing soda, in preference to that with potash; because the hydrate of soda, on account of its smaller atomic weight, contains more water, and consequently more oxydizing material than the same weight of the hydrate of potash. Also the mixture of the hydrate of soda and lime attracts moisture less readily than that of potash and lime; and moreover, the hydrate of soda requires but twice its weight of anhydrous lime to form a mixture that will not melt at a red heat. For one part of potash three of lime are necessary. The easiest method of preparing it, is to slake the lime in a solution of known concentration, of either of the above alkalies; heat the mixture to redness in a crucible, and rub it to a fine powder. Or we may rub up quickly one of the alkalies (first melted and then cooled) in a warm mortar, and then mix intimately with it anhydrous lime in the proper proportion. In this latter case, the lime must be first slaked and then heated to redness as a fine powder before being used. The mixture prepared in either of the above ways is once more heated to redness to drive away all mixture, and then preserved in a well stopped phial."

In performing an analysis, the process is as follows:

"The burning tube, clean and dry, is half filled with the alkaline mixture, and that is the measured quantity to be mixed with the substance about to be analyzed, which is first dried and weighed in the ordinary method. The quantity of the substance employed should vary according as it contains more or less nitrogen. When rich in this element about three grains, but when its quantity is quite small about six grains should be used."

"The mixture of the weighed substance with the measured quantity of the alkaline power, is done in a porcelain mortar with a flat

bottom, (which has been previously slightly warmed,) and with a very gentle movement of the pestle. If these precautions are strictly observed, no loss of material will result by its sticking to the sides of the mortar, or bottom of the pestle. If the mixture be pressed too hard, or if it be rubbed too fine, or if the mortar be not perfectly dry, a portion of the substance will adhere to its sides. After introducing the mixture into the burning tube in the ordinary way, the mortar is rinsed with the alkaline powder, with which we fill the tube to within an inch of its mouth, in which is placed a loose stopper of asbestos that has been heated to redness and cooled."

The use of the asbestos is to prevent the gas that is generated, from projecting any of the powdered alkali into the apparatus containing the hydrochloric acid, which accident would of course cause a serious error in the result.

"With a tight cork we unite the tube containing the mixture to the acid apparatus, and by warming slightly the bulb *a*, by bringing an ignited coal beneath it, we are able to ascertain whether the apparatus is tight, for if so, the liquid will be chased from the bulb *a*. The tube is now heated as in the case of ordinary organic analysis, by placing ignited coals at the front part of the furnace, that contains none of the organic substance. The cork must be kept as warm as possible, so that it may contain no moisture, which by absorbing ammonia, may cause a loss of nitrogen. So soon as the first part of the tube is red hot, we carry the fire slowly back upon that part containing the substance."

"Carbonic acid is formed, the oxygen in the water of the hydrated alkali combining with the carbon of the substance, the free hydrogen combines with its nitrogen, and ammonia is formed, which is absorbed by the hydrochloric acid. Hydrogen or carburetted hydrogen (according as the substance contains more or less carbon) is evolved, and this passing through the acid without being absorbed, enables us to see the progress of the burning."

"It may be well to remark, that a continued current of gas is evolved, but there need be no apprehension of the escape of the ammonia, for its absorption is so complete and goes on with such rapidity, that one has rather to apprehend a recoil of the fluid; if the current of gas is only stopped for a moment, the fluid rises in the bulb *a*, and if the fire be carelessly attended to, it will enter the tube *d*, and thence the burning tube, rendering the experiment useless."

Drs. V. and W. state that when the body contains a large portion of nitrogen, this accident is more to be apprehended, and they recommend the mixing of an equal weight of some substance free from nitrogen, as for instance, sugar with the body containing nitrogen, and with the alkali lime, which by forming gases not absorbable by the acid, will cause no error, and prevent any accident.

"After the whole length of the tube is red hot and gas no longer generated, when all the carbon of the substance is burnt, and the mixture in the tube appears white, the small end of the burning

tube is broken, and a quantity of air drawn through the burning tube, and absorption apparatus, in order to bring all the ammonia in the tube in contact with the hydrochloric acid; the air is drawn through by means of the extremity *g*, a small tube containing potash being placed between it and the mouth, to prevent one from inhaling the acid vapours."

"We proceed to analyze fluids exactly as in the burning bin-oxide of copper. A little of the alkali lime is first introduced into the burning tube, and upon that is dropped the little bulb, containing the known quantity of the fluid to be analyzed, with its capillary extremity broken off, and then fill the tube as before with the alkali lime. The operation goes on more regularly, if we commence by heating the first third of the tube to redness, and then warming that part of the tube containing the bulb, from which the fluid is expelled, which disseminates itself over the middle part of the tube, without being decomposed; if we now carry the fire from before backwards, a gentle and constant current of gas will be evolved."

"After the burning is complete, and the air passed through the apparatus, the contents of the absorption apparatus are emptied into a small porcelain capsule. With a dropping tube we introduced first into the apparatus, a mixture of alcohol and ether, which must be shaken backwards and forwards, and then added to the first fluid. The bulbs are now washed with water, until it affords no acid reaction, and all the washings added to the hydrochloric acid containing the hydrochlorate of ammonia. The washing with alcohol and ether has for its object the dissolving of any carburetted hydrogen which may be formed during the burning, and by collecting in the bulbs would prevent the water from coming perfectly in contact with the sides, and thereby rendering a complete washing difficult. Seldom is more than an ounce to an ounce and a half of fluid needed for the removal of all the hydrochlorate of ammonia."

"To this fluid, now containing hydrochlorate of ammonia, an excess of a pure solution of the chloride of platinum is added, and it is then evaporated to dryness, either first by the aid of a spirit and then by a water bath, or entirely by the latter means. From a well conducted burning, the chloride of platinum and ammonia that is obtained is always beautifully yellow; but if the material contained a great deal of carbon, or was burnt with difficulty, then the chloride of platinum and ammonia is of a darker colour. This colour though, has little or no effect upon the accuracy of the result supposing the washing to have been carefully done."

"Upon the residue in the porcelain capsule, after it has cooled, a mixture of two parts of absolute alcohol and one of ether is poured. This dissolves the excess of the chloride of platinum, and no part of the chloride of platinum and ammonia. It is immediately known, whether there has been an excess of the chloride of platinum, by the fluid assuming a yellow colour; if it is colourless, then there has been a deficiency."

“ The washing of the residue is performed easiest by holding the capsule, after the fluid has been poured upon a weighed filter, perpendicularly over the same, and washing the precipitate in, with the solution of alcohol and ether. The filter must be washed by the same mixture until it passes without colour or acid reaction. The precipitate and filter are dried on a covered capsule or weighed tube at 212° Fahr., and weighed. It is well, in order to continue the result, to decompose the chloride of platinum and ammonia by heat, and out of the residue obtained, calculate again the quantity of nitrogen, and if the chloride of platinum was pure, the quantity of nitrogen calculated by this latter method will not differ from the double chloride.”

In decomposing the chloride of platinum and ammonia, it must be done by heating it, enveloped in its filter, gradually to redness; for if it be done too hastily, the vapour of the muriate of ammonia and chlorine will remove mechanically some of the platinum. The chloride of platinum used in this process, must be perfectly pure, and Drs. V. and W. state, that it is difficult to obtain spongy platinum without a trace of muriate of ammonia, by simply decomposing the chloride of platinum and ammonia by heat. They therefore recommend that the spongy platinum used to form the chloride, should be well washed with hot distilled water, before being dissolved in the nitro-hydrochloric acid.

“ If we weigh the nitrogen as the chloride of platinum and ammonia, we have one hundred and seventy-seven parts of it for every two thousand seven hundred and eighty-eight of the double chloride, but if as metallic platinum, one hundred and seventy-seven parts for every twelve hundred and thirty-three of the metal.”

“ The weight of the chloride of platinum and ammonia remains constant under a long continued drying at 212° Fahr., as well as that of the filter, after it has been completely washed. If there be a trace of acid remaining on the filter it becomes dark and friable.”

I believe that now all the important steps in this operation have been fully detailed, and I shall conclude with a translation of a letter of Prof. Liebig's, that was attached to the original publication, in order that it may be seen, how highly this new method of estimating the quantity of nitrogen in organic bodies is esteemed by that distinguished chemist.

“ I have had the pleasure of being constantly present at the development of the experiments by which Drs. V. and W., by degrees, have arrived at the method, which they have here described. I regard this new method of estimating nitrogen as one of the most important improvements in organic analysis, because it determines the quantity of nitrogen with a certainty and precision which until now were wanting. The whole operation is completed in a few hours, and with all the accuracy of the determination of the carbonic acid. I doubt not that this apparatus will very soon replace the ordinary method, to the contentment of all analytical chemists.—J.L.”

The little experience and observation I have had of it, verify the opinion of Prof. Liebig, and in simplicity it is every thing that can be desired.

[“American Journal of Science and Arts.”]

On the Climate of the Coal Epoch. By ROBERT HARKNESS, Esq.*

[Read before the Manchester Geological Society, April, 1843.]

IN the whole range of physical science there is no circumstance more calculated to excite our astonishment than the discovery of SMITH; that all the deposits, which constitute the solid crust of the globe, are characterized by their own peculiar fossils. This discovery, which the most fertile imagination would have failed in anticipating, has justly entitled its author to the appellation of the “father of English geology.” By these extinct organisms we have recorded the various physical conditions of the earth during periods long prior to the creation of the human species: and from these natural characters we learn that many and great changes have taken place on our globe during the immense geological epochs, of which the mind of man can form no estimate. These records, written by a hand that cannot err, tell us of eras when the land of our own, and even more northerly regions, was clothed with a vegetation vying in luxuriance with that which is at present found only mid the hot and humid savannahs of the tropics. From these natural hieroglyphics we also learn that during the epoch which appears to have immediately preceded the creation of the human species the general climate of the earth possessed to a great extent a frigid character.

The observations of Smith, above alluded to, on the geological distribution of extinct species of organisms induced him to conclude that the relative portion of each geological formation may be ascertained solely from the fossil remains which it affords. This conclusion is now received as an axiom in geology, and is of the greatest use in determining the geological antiquity of the various stratified deposits.

Besides the index which fossil remains afford of the relative position of deposits, there is another circumstance equally interesting which they record; that is, the condition of the earth’s surface during the period of the deposition of the strata which they indicate. The physical history of the earth which we learn from these monuments is so interesting, that a consideration of the condition of our globe during the epoch of the coal formation, has always excited the greatest curiosity. In this formation we have the first direct evidence of the existence of terrestrial vegetation: and such fantastic forms has the flora of this epoch assumed, when compared with our own time, that its narration has been termed “the romance of natural history.”

* Communicated by the Author.

But perhaps the circumstance most calculated to excite our astonishment, is the uniformity of climate which appears to have obtained during the earlier geological epochs. This circumstance we learn from the geographical distribution of extinct species of organisms, which seems to have been much more extensive in the earlier epochs than in the more recent formations.

In the silurian formation we find the same fossils extended over a large geographical area. In Russia, Murchison and Verneuil have found fossils in this formation identical with those of the same formation in England. Amongst those enumerated were the *Leptæna depressa*, *Leptæna sericea*, *Lingula Lewisii*, *Orthis canalis*; and, according to M. Eichwald, two or three similar species of *Trilobites* are met with. But the Trilobite *Calymene Blumenbachii* has the most extensive geographical distribution, for it is found not only in the silurian limestone of Europe, but also in that of America.

In the Devonian, or old red sandstone, of Russia, the same geologists observed organic remains similar to those which this formation affords in the British isles. Amongst these are the *Holoptychius nobilissimus*, and also other forms of fossil fish, which occur in this deposit in Scotland; associated with the same species of *Orthis*, *Productus*, *Spirifer*, and *Terebratula*, which are found in this system in Devonshire, Westphalia, and Belgium.

The carboniferous limestone in Russia also contains the same marine remains which are met with in this formation in our own country. One of these, which is common to the lower beds of limestone of England and Scotland, the *Productus hemisphericus* occurs in the same bed in Russia. Some of the other are the *Productus Martini*, *P. punctatus*, *Sanguinolaria sulcata*, *Spirifer Mosquensis*, *Cardium alæforme*, *Cidaris vetustus*, *Chætites radians*, and the *Lithostrotion floriforme*; all of which are common to the mountain limestone of England.

If the extensive geographical distribution of marine remains, during the earlier geological epochs, were the only evidence of the uniformity of climate, it would be of comparatively small value; for the medium in which marine remains existed is, in relation to heat, of such bad conducting power, that the sea is, when compared with the land, of an almost uniform temperature. But in the formation which succeeds the carboniferous limestone, we find a tribe of organic remains which were not only themselves more susceptible of heat or cold, but which were also exposed to a medium which is rapidly influenced by any change of temperature. From these remains, which are so abundant in the coal formation, we are enabled to derive the greatest information of the general temperature of the earth during this epoch.

We find that the fantastic forms of vegetables which existed in this case have had an almost universal distribution. Not only has our own country, during the period of the formation of coal, been clothed

with tropical forms of vegetables, but what is more striking, even those regions which are at present enveloped almost the whole year in an icy covering, have, during this epoch, been the abode of similar forms of vegetation. Melville Island, for instance, situate in latitude 75° north, contains in its coal deposits similar plants with those which are met with in the coal mines of England.

M. Eugene Robert, in his report on the geological observations made during the French northern nautical and scientific expedition, in 1838-9, remarks, that the Spitzbergen coal formation also contains fossil plants, analogous to those in the coal beds of our temperate latitude, but no impressions of ferns.

During the epochs which succeeded the coal, the physical conditions of the earth appear to have undergone a great change. The climate seems to have been much less uniform; and the geographical distribution of species of organic remains restricted to a much smaller area than during the preceding formations.

In the new red sandstone fossil plants are found; but these have, in a great measure, lost their tropical aspect. When we come to the chalk formation, the change of climate has been so great that, in comparatively small areas, entirely different species of organic remains occur. In the chalk of Egypt the species are so different from those of our own country that it would be impossible to ascertain the nature of the formation by comparing the fossils with those of the chalk of Great Britain. Even on the southern declivities of the Alps the fossils of the chalk are by no means the same as those which occur in this formation in England. In the chalk of America, according to Bruyer and Conybeare, of 108 fossils which this deposit contains, only two are identical with the fossils of this formation in Europe.

Many causes have been cited to which to attribute the uniform tropical climate which appears to have prevailed during the earlier ages of our globe; but only two of these have been to any great extent generally adopted. The one, which has for its object the explanation of the occurrence of vegetables of a tropical aspect in the frigid zone, supposes that the earth's axis, during the epoch of the coal formation, had a different position from what it has at the present time. The other, which is intended for more general application, refers the uniform tropical climate to the influence of the internal heat of the earth.

The first of these, which has been propounded to explain a comparatively local circumstance by changing the position of the earth's axis, is contradictory to other geological facts; for if the earth's axis had been displaced, and the polar regions converted into intertropical regions, (for no other permanent axis could the earth assume, owing to its form being that of an ellipsoid of revolution, *but one in the diameter of the plane of the equator,) then one, if not more, of the many coal fields which are found scattered over the surface of the earth would be placed within the polar regions by the change of

position of the axis of rotation which is supposed to have taken place previous to the coal deposit. For we find coal occurring not only in Europe, and the frigid zone, but in China, the Birman Empire, India, Persia, and also very extensively in various parts of America, and many other parts of the earth. There is another circumstance which shows that a change in the earth's axis is impossible. This is the elliptical form, both of the internal strata and also the external surface of the earth, having coincident centres and identical axes—"a state of things," to use the words of Sir John Herschell, "which is incompatible with a subsequent accommodation of the surface to a new and different state of rotation, from that which determined the original distribution of the component matter."

The theory which attributes the uniform tropical climate of the coal epoch, to the influence of central heat, appears, at first sight, to be more probable than that which refers it to a change in position of the axis of rotation. But this theory is invested with insuperable difficulties: and the very existence of the cause is at best merely probable. Even should the internal part of the earth be in a fluid state, owing to central heat, a supposition which is supported by the ellipsoid form of the earth, which appears to indicate original fluidity; yet such is the bad conducting power of the earth's crust that the internal heat is unable to melt the snow at the poles, although nearer the source of central heat than any other part of the earth's surface. If this cause does exist, the radiation of heat from the earth must be going on even at the present time, and, consequently, a contraction must be taking place in the magnitude of the earth; but so slowly does this cause appear to operate that, since the time of Hipparchus, nearly two thousand years ago, the length of the day has not decreased in the smallest perceptible degree, which would have been the case had the internal heat undergone the slightest change during this period.

It is most probable, that the internal heat of the globe exerted as little influence on the general climate of the earth, in the epoch of the coal formation, as it does at the present time; for had it existed to a greater extent than at present, it would have destroyed the vegetation then existing in the frigid zone, rather than have supported it. In this part of the earth, where for the greatest portion of the year the sun never appears above the horizon, the coal plants would be exposed to a temperature which would cause them to exercise their vital functions under peculiarly injurious circumstances, namely, under the absence of solar light. This want of light when accompanied by the influence of central heat, would in a very short time destroy the vegetation entirely. For vegetables are so constituted that when exposed to heat and deprived of light, the products of their respiration are altogether changed, and in a short while their tissue becomes disorganized, in consequence of the evolution of carbonic acid gas, and the vegetables themselves perish for want of the presence of solar light. So great is the dependance of vegetation

on solar light and heat, that no theory will fully account for the extensive diffusion of vegetables of a tropical aspect over the surface of the globe, unless it be in a great measure based on solar influence.

As before stated, the constitution of vegetables is such, that unless exposed to the presence of solar light as well as heat, they not only cease to grow but even to exist. So great an influence has light over them, that they appear to owe their organization almost entirely to its power. For no sooner do the first leaves develop themselves, than they commence absorbing carbonic acid gas from the atmosphere; and appropriating the carbon to the formation of their woody tissue. If however, they are deprived of light, this absorption of carbonic acid gas not only ceases, but they exhale this gas and lose a portion of the carbon designed for the formation of their internal structure. Liebig and others are of opinion, that the carbon which is the most abundant elementary principle in plants, is derived entirely from the atmosphere; and this appears to be one of those general laws which is not merely applicable to plants when under the fostering care of man, but of that class which is applicable to all circumstances, and in all times. We have in this law, a proof of the great importance of solar light, in the development and growth of vegetables.

Since plants derive the greatest portion of their solid structure from the atmosphere, it follows that during the earlier epochs, and up to the period of the deposition of the new red sandstone, the atmosphere which then surrounded our globe, must have contained a much greater quantity of carbonic acid gas, than it does at the present time.

For the immense beds of carbon which constitute the coal deposits, must have originally existed in the atmosphere, in the state of carbonic acid gas, and have been abstracted from thence by the plants, which now form the beds of coal, which are composed of compressed and chemically altered vegetables. This supposition is in a great measure confirmed by Brongniart, who is of opinion that, owing to the large quantity of carbonic acid gas diffused through the atmosphere during the deposition of the coal strata, the air was unfit for the respiration of terrestrial animals. For it is in the formation which immediately succeeds the coal, that the first traces of animals respiring atmospheric air occur, in the form of the *Labyrinthodon*; a circumstance which appears to indicate that a very great change had taken place in the physical condition of the earth, immediately prior to the epoch of the new red sandstone.

The effect of such an atmosphere as that which existed during the formation antecedent to the new red sandstone, would be the production of a much more luxuriant vegetation than that which at present clothes the surface of the earth. For Saussure found that plants grow with greater vigour in proportion to the quantity of carbonic acid gas which the atmosphere contains, provided that quantity does not exceed one-twelfth of the whole. This however, would not be the only result, for in proportion to the quantity of

carbonic acid gas, which the atmosphere contained, so would its density be increased, and hence its influence on solar heat would be very different from that of our present atmosphere.

It is a well known law of physical science, that liquids, and probably all substances, that do not undergo regular crystallization, are more pervious to heat according as they possess a greater refractive power. Now this refractive power is entirely dependent on density, and as we have seen that in the earlier geological epochs, the atmosphere was much more dense than our present atmosphere, it must of necessity follow, that during these epochs, the atmosphere must have been more pervious to heat than at the present time. Here we have an explanation of the cause of a tropical climate during the deposition of coal strata, without having recourse to the extravagant supposition of a displacement of the earth's axis ; or the almost equally gratuitous hypothesis of central heat.

If the greater perviousness to heat were the only effect of an increase of density there would still be great difficulty in explaining the origin of the, comparatively speaking, uniform temperature of the globe during the earlier geological epochs. But an increase in density renders the atmosphere less pellucid, and, consequently, its capacity for heat is greater ; whether that heat be derived from the direct influence of the sun or from the radiation of the earth. For Lambert found that the capacity of the atmosphere for heat varies according to the same law with its capacity for absorbing a ray of light passing through it from the zenith ; which absorption depends entirely on the density of the medium through which the ray passes. The result of such a law would be to modify the extremes of heat or cold, and, consequently, the production of a more uniform temperature. At the present time, during that period of the year when the solar rays fall most obliquely on any portion of the earth's surface, that portion loses, by radiation into space, more heat than it acquires by absorption, But in the epochs antecedent to the deposition of the new red sandstone, that portion of heat which is now radiated into space was absorbed by the atmosphere, owing to its then possessing a greater capacity for heat than at present, and consequently would maintain a more uniform temperature on the earth's surface.

The internal structure of such coal plants as retain traces of vegetable tissue, indicates an equable temperature, such as would be produced by an increased density of the atmosphere. Witham, in his treatise on the internal structure of fossil vegetables, concludes, from the coniferæ of the coal formation having but slight appearances of concentric layers, a circumstance of common occurrence amongst the tress of our tropical regions, that the variation of temperature was less or at least not so abrupt as during our own time.

From the foregoing observations it is evident that from the first dawn of animal life, and probably also during the countless ages which preceded the creation of organized beings, till the period when terres-

trial animals were first called into existence, the earth possessed a warmer and a much more equable climate than at the present time. This equable climate appears to have resulted from the great density of the atmosphere which then surrounded the globe; a density originating from the great quantity of carbonic acid gas diffused through the atmosphere, and destined to support that luxuriant vegetation which clothed the surface of the earth during the coal era, and which is now deposited amongst the strata which constitute the solid crust of the globe. This dense atmosphere, from its capacity for absorbing heat, prevented, during the period when the solar rays fall most obliquely on any part of the earth's surface, the dissipation of that heat acquired by the earth during the time when the sun's rays fall most directly, and consequently prevented the occurrence of that degree of cold which is so common to our present climate. By this means most probably have regions which are, at present, clothed with ice, been rendered fit for the abode of plants, which indicate a tropical climate; and to this cause may be attributed that uniform temperature which existed during the earlier geological epochs, and which we are justified in supposing to have prevailed from the extensive geographical distribution of analogous forms of extinct vegetables.

Description of a Bridge of Béton, (Concrete,) constructed at Grisoles, in the Department of Tarn et Garonne, in France.† By M. LEBRUN, Architect, of Montauban.*

Living in a region where suitable building stone is scarce and expensive, and where brick masonry alone is used, M. Lebrun, guided by the fine works of M. Vicat, on hydraulic limes, conceived the idea of substituting for this masonry the *beton*, which the Romans used with so much advantage.

In consequence, he submitted, in 1839, to the Minister of Public Works, the project of a bridge entirely of *beton*, which he offered to construct on the lateral canal of Garonne, to be traversed by many of the royal and departmental routes. This offer having been accepted under certain conditions, M. Lebrun commenced his work in June, 1840.

* The French *beton* is nearly identical with the English *concrete*, the main difference being in the manipulation; thus *beton* is composed of lime, sand, and small pebbles, or broken stone, *taken separately*, and successively mixed together, the pebbles being added last; while *concrete* is usually formed of lime, mixed directly with gravel, *containing naturally* about the due proportion of *pebbles and sand*; proper quantities of water being used, and the factitious stone resulting, in both cases, being in effect the same.—TR.

† Béton, or concrete, has before been used in retaining walls and other constructions, and, as is stated by Gen. Pasley, of H. B. M. corps of Engineers, (in his admirable Treatise on Calcareous Cements,) it was also applied experimentally to build a military casemate near Woolwich, of which the arch had 18 feet span,

I.—*Selection and Preparation of the Materials.*

The *lime* was of the hydraulic quality, burnt in perpetual kilns, by pit coal. The *sand* was clear of all earthy particles, of fine grain, and pretty uniform. The *gravel stones*, of the size of a hen's egg, came from the River Garonne. The lime was slaked alternately in two basins, joined together. For this purpose, we poured at first, in one of the basins, a quantity of water proportioned to that of the lime which we wished to slake; we then put in sufficient quicklime for the water to cover it; then we left the lime to slake freely without disturbance, except by taking care to prick it, from time to time, with a stick, to introduce the water into those parts of the basin where the dissolved lime was dry. When the fermentation had ceased, we stirred up the lime in every direction with an iron hoe, in order to mix the paste, and render it homogeneous; we left it then in this state not to be used for twelve hours after slaking.

The proportions observed by M. Lebrun for concretes (*bétons*) destined for the construction either of walls or arches, were, in every ten parts, composed of two parts of lime in paste, three parts of sand, and five parts of gravel stones, or pebbles.

For making the mortars, we placed on a paved surface, two measures of the slaked lime, which, after having been well beaten with pestles of cast iron, softened again by yielding up a part of the water with which it was charged; then we placed beside it three measures of sand, which we mixed little by little, with the lime, always having the aid of the pestles, and stirring the whole with the shovel and hoe, in order that all the parts of the sand should be well incorporated, observing not to put any water into the mortars, but if the sand was too dry, we moistened it, a few moments before mixing. As soon as the mortars were sufficiently manipulated, we

5 feet rise, and 6 feet depth at the crown, and which when subjected to the direct fire of 24 pounder guns, as well as the vertical plunge of 13-inch shells, loaded to weigh 200 lbs. each, resisted both with success, and, contrary to expectation, was less injured by the latter, than by the former.

General Treussart, of the French army, after mentioning the successful construction of several concrete vaults, recommended this material for aqueduct bridges and for the revetments of fortresses, in certain situations.

Nevertheless, the bridge above described is amongst the first, if not the very first, construction of that nature which has been successfully executed for ordinary use, and its success will suggest at once to the mind of the American engineer, many situations, in the southern section of the Union, at least, where concrete may thus be used with great propriety and economy; indeed its comparative cheapness, in some cases, would fully justify a fair trial of it, even in the northern United States, though the severity of the winters, there, would seem to render its success in that region more problematical.

We must, however, observe, that the failure of the concrete wharf walls, at Woolwich and Chatham, in consequence of tidal exposure, and the necessary protection of the concrete sea wall at Brighton, with woodwork, to shield it from the action of water in mass, (as mentioned by Gen. Pasley,) points out the necessity of confining the application of *concrete* to constructions, within reasonable and proper limits.—TR.

added five measures of gravel stones; the whole was then long and forcibly mixed and pounded, until each part of the gravel was sufficiently enveloped by mortar: then the *betons* were taken in quantity, to wait for the moment of being used. We took care to make only what we could employ in a day's work, without which precaution it would have lost its cohesion.

II.—*Construction of the Abutments.*

The 15th of June, 1840, the excavation of the foundations of the two abutments being done, we commenced laying the *betons*, taking care, each time that a layer, or course, was finished, to cover it up immediately with wet mats of straw, to prevent a too rapid drying by the heat of the sun. By means of this precaution, the new course connected itself more intimately with the one below. We continued the masonry all of *beton*, (the backing of the arch and abutments keeping pace), until reaching the height fixed. The exterior faces of the abutments, (not next the earth), and of the walls, were formed by some planks strongly fixed, against which the *beton* rested. These planks were removed, two or three days after, and the faces of *beton* remained exposed, and were very well preserved. At the height the springing of the arch, we laid five courses of bricks plumb on the faces of the abutments, to serve as perpendicular faces for the centre to fit up against, and enable it to detach itself easily.

III.—*Construction of the Centre.*

Fifteen days after the laying of the last *betons*, we commenced the construction of the centre, composed of many courses of bricks, laid flat, in succession, (from the spring towards the crown), following the curve of the arch at the intrados, built partly with plaster and partly with cement, or hydraulic mortar, and supported at the springing by projecting masonry, or by a timber for that purpose. The centre was formed of four courses of bricks, in thickness or, depth, say nine inches; the three lower were laid with plaster, and the upper course with cement, to shelter the plaster from the dampness of the *beton*. The upper bricks of the centre were covered by a bed of mortar of clay, in order to model perfectly the intrados of the arch, and to hinder the *beton* from forming one body with the bricks.

The construction of the centre being finished on the 17th of August, we established, three days after, the masonry of the two heads of the arch of brick, (in lieu of ring stone,) which were completed on the 26th of the same month.

IV.—*Construction of the Arch.*

Immediately after the construction of the two heads, we wrought them into the general mass of *beton*, forming the arch; this operation was finished on the 5th of September, with the exception of the backing up, which was accomplished, on both sides, the 11th of the same month. The *betons* of the arch were composed in the

same manner as those of the abutments, and manipulated by the same process; but we added $2\frac{1}{10}$ cubic feet of cement for every $35\frac{1}{2}$ cubic feet, or $\frac{1}{17}$ th, of the mass, to augment the strength of the mortars of the body of the arch. This construction was made without following any regular order, and the *betons* were cast in masses upon the centre, to the thickness of two feet, which formed the first general bed, or layer, on the development of the arch. This first bed being finished, we formed the second in order to reach the thickness of three feet at the key, the spandrel backing, and the abutments being levelled up. A coping of hydraulic mortar was then placed over the whole extent of the arch, and covered immediately with a layer of clay, strongly beaten.

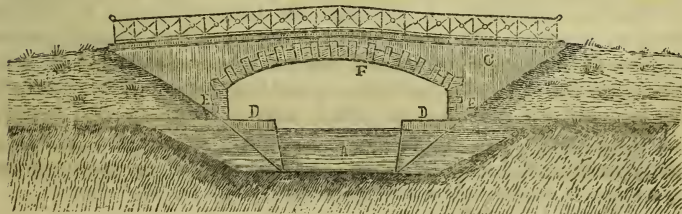
V.—*Decentrement.*

All was left in this state until the 25th of January, 1841; we then proceeded to the operation of the decentrement of the arch. The 28th of January, the centre of bricks was taken away, and the intrados of the arch appeared very even in all its parts. After three months, it manifested not the smallest settlement in its masonry, and, since then the bridge has stood through the summer, without incurring the least degradation capable of affecting its solidity. This bridge as a clear opening of $39\frac{1}{2}$ feet between the abutments; the middle is placed in the axis of the canal, which has two towing paths; its breadth is $19\frac{2}{3}$ feet between the heads, or faces of the rings; and the arch is formed of a segment of a circle of $39\frac{1}{2}$ feet chord, and $5\frac{1}{4}$ feet rise, or versed sine.

The entire mass of the abutments is of *beton*, except the four angles on the sides of the towing paths, which are of large stone, rounded on the arris, on account of the rubbing of the towing lines. The arch is also of *beton*, as are the faces of the tympan, or spandrels, and the intrados, with the exception of the arrises of the soffit, or rings of the heads, which are of brick masonry.

M. Lebrun has joined to his memoir, along with a plan of the bridge of Grisoles, many certificates, from the mayor of this commune, and from the engineer of the lateral canal of the Garonne, proving the complete success of the works, and the solidity of the construction, which has endured the proof of the passage of loaded carriages, the numerous influences of heat, and some very severe frosts, without having suffered the least degradation.

VI.—*Explanation of the Figures.*



a, canal.

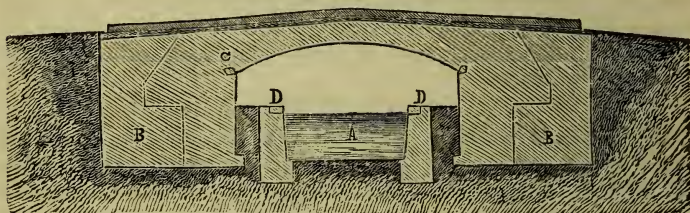
b, abutment of *beton*.

c, mass of arch, also of *beton*.

d d, towing paths.

e, angles of bridge upon the tow-paths, built of large stone, with the angles rounded.

f, arises of the soffit, or bands of brick at each head of the arch, instead of the usual ring-stone.



Upon Impressions or Pictures produced by Electricity.

By G. KARSTEN.

(From Poggendorff's "*Annalen der Physik*," vol. lviii, p. 115.)

FROM continuing my experiments on electrical images* I have arrived at the following results. 1st. I have been able to establish more accurately the conditions under which successful images are obtained. 2ndly. There is a difference observable according to the electricity employed. 3rdly. The electrical pictures are essentially different from those produced by light, whereas they appear to be identical with those produced by what Professor Moser terms the invisible rays. 4thly. The production of these images by galvanism is yet in doubt; magnetism, however, does produce them. 5thly. These pictures are produced by heat, and that when applied in various manners; the conditions, however, under which they are thus called forth, are not as yet clearly made out.

It has been already stated in the former paper, that images are produced with distinctness in glass, only when the sparks strike off from the coin into the metal plate on which the glass rests. This I am enabled to confirm, with the addition that the image is so much the more distinct the more frequently the spontaneous discharge of this coated plate, formed by the coin and the plate of metal, takes place. This appeared to indicate that a continuous current of electricity is required for the production of distinct images, a supposition which is borne out by the conduct of imperfect conductors, upon which the images were produced with great precision, the electricity escaping thence but slowly. The coin upon these occasions appeared encircled with a crown of rays, arising

* These images are produced by electrifying a coin or medallion, whilst placed on a pane of glass or on metallic plates; removing the coin, and then breathing on the pane or plates.—"*Annals of Electricity*," Vol. X., p. 454.—EDIT.

from the continuous nature of the discharge. In experimenting upon metal plates this operation must be reversed. If it was requisite when employing plates of glass to get rid of the electricity and prevent its accumulation thereon; so on the other hand, when operating on metal plates, was it necessary to reduce the velocity of the electricity. As observed in my preliminary notice, I at first had recourse to a piece of oiled paper inserted between the coin and the plate; I now, however, make use of a thin piece of glass, or, what is far preferable, a film of Siberian mica: by this means, with not more than 15 or 20 turns of the machine, that is to say, in an interval of from 10 to 15 seconds, I obtain an extremely distinct picture. The thinner the insulator is, or in other words, the more completely the opposite electricity is disguised, the more distinct the image is, and the more rapidly is it obtained. The following experiment may be adduced in proof of this: Two films of mica were laid upon a metallic plate in such a manner that at the centre they formed a double layer, whereas towards the edge the layer it was but single. After 15 turns of the machine, a coin laid on this plate gave a very distinct image towards its circumference, whereas the central portion came out but weakly. The most precise images are obtained likewise here when the spontaneous discharges of this miniature battery follow in quick succession. Since the image which is formed by the passage of the spark from the positive conductor is, in point of fact, to be attributed to the negative electricity disguised on the metallic plate, I shall in the sequel term this a **NEGATIVE ELECTRICAL** image: on the other hand, the picture obtained upon a negative conductor, I shall call a **POSITIVE ELECTRICAL** image. This of course applies only to pictures formed upon metal plates. From these experiments, therefore, we draw the inference that it is a requisite condition for the production of distinct images, that the velocity of the electricity should be modified.

Various modifications of the experiment were made, both with regard to the plates, and likewise the objects to be figured. By way of plates, there were employed white and coloured glass, marble, porphyry, granite, syenite, satin-paper, wood, horn, resin, silver, copper, German silver, tinsplate, steel, zinc, and various metallic alloys. The phenomena were very remarkable when a polished plate of Diorite from Sweden was employed for the experiment. This substance consists of a mixture of albite, hornblende, and interspersed portions of magnetic iron ore. When a film of mica was inserted between the coin and the plates the image became visible only on the metallic points scattered about on the stone. When no film of mica was inserted between them, the picture was produced only on the albite and the hornblende; not, however, with equal distinctness on each;—on the magnetic iron ore there was no image at all. Something similar may be observed with all compound minerals. We here see very evidently that the conducting power of the various substances is not to be overlooked. Perhaps we shall

thus have it in our power to ascertain the relative conducting power of certain bodies. Nor were the objects to be figured less varied than were the plates. Coins and medals of various metals were employed, as were also seals, cut stones, other minerals, and likewise glass, and paper. The film of mica inserted between the coin and the plate never failed in being given with remarkable precision. Here also minute distinctions were rendered accurately; and this was carried so far that the paper impression from a divided circle engraved upon copper, was reproduced with precision upon a metallic plate with its fine divisions. Generally, however, I made use of various pieces of money for my experiments, on account of the equality of the stamp. On producing an image on a plated-silver plate, similar to those employed for Daguerreotypes, this extraordinary circumstance presented itself, namely, that after a great number of turns of the machine, the image of the coin and of the film of mica were distinctly impressed upon the plate as it was, without the employment of any vapours; and this engraved coin, as it were, presented itself in dispersed light with a brown, and in reflected light with a bluish-white colour. I at first attributed this to traces of iodine remaining on the plate, or to those of other substances employed in polishing it. Hyposulphite of soda, however, or diluted nitric acid, did not alter the image in any way. In order to make certain, however, I employed perfectly new plates, which presented the same phenomena; with 1000 turns of the machine the whole picture was, as it were, etched into the plate; and with 2000 turns the intensity of the brown colour was heightened. A perfectly new silver coin had apparently lost its polish at those parts whose image was impressed on the plate; yet the notion that any metallic particles could be transferred from the coin to the plate is quite inadmissible, owing to the insertion of the film of mica between them. By the formation of sparks between the coin and the mica film on the one hand, and the metal plate and the coin on the other, it is possible that very minute quantities of nitric acid might be formed; it nevertheless does not appear admissible to attribute these effects to such a cause, inasmuch as the colour of moistened litmus paper did not undergo any visible change. I have in my subsequent experiments often observed a repetition of these phenomena, but only when plated or metal plates were used; at any rate, it appears to me a process altogether independent of the formation of the image, inasmuch as the latter is already produced in the greatest beauty after 20 turns, whereas many turns are required for etching an image thus into the plate.

II. In the first notice above referred to, I stated that the effects due to $+$ or $-$ electricity were similar. As yet, however, nothing is satisfactorily established on this head. In so far it is true, that the effects from both kinds of electricity are alike, namely, that the application of aqueous vapour brings out with equal distinctness, and that either on glass or on metal, the image produced either by

+ or — electricity, or by a combination of both. I have not been successful in imparting to the coin directly, precisely equal quantities of the two electricities, inasmuch as my machine has separate conductors, and the effect from the + conductor is much stronger than the other; but I could observe no difference on employing the sparks drawn from the outer coating of a jar provided with a Lane's discharging electrometer. Aqueous vapour, the original application I had recourse to when I first obtained distinct pictures, (and up to now the best,) showed decidedly no difference; upon every occasion the raised portions of the coin remained unwetted. On breathing upon the plates one could almost make out two kinds of breath. That is to say, one remarks, first, that the breath spreads itself uniformly and with rapidity over the entire plate; and, secondly, that at certain places, especially where the image lies, it is absorbed, as it were, by the plate, a phenomenon much resembling that presented by water when it extends itself between two plates of glass almost in contact with each other. Besides, the first breath disappears rapidly, whereas the second remains attached to the metal a long while. Although there is a great simplicity between the electrical pictures brought out by aqueous vapour, nevertheless there is a difference to be observed; as was rendered evident upon fixing them by the vapour of iodine or of mercury. We sometimes obtain images in which the salient parts of the coin are more affected by the vapour than the rest; sometimes the reverse of this is the case. The first kind of picture presents the effect of a coin in relief; the second kind, that of a coin in intaglio. At first there appeared to me to be no rule whatever for the development of the one or the other kind of image. The distinction between positive and negative pictures, which in the opinion of Professor Moser is, in the light-pictures, to be attributed to the tension of the vapours, does not, for a certainty, obtain, with respect to the electrical pictures. I subsequently remarked, however, that a *negative electrical* picture, produced by a few turns of the machine, gave, when fixed with the vapour of iodine, a negative or intaglio image; a *positive electrical* figure, on the other hand, gave a positive image; in other words, one in relief. It is remarkable, how in this case the terminology ordinarily employed in electricity, adapts itself to the name introduced by Herschel in a totally different field of enquiry. The precision of the pictures was no ways increased when the iodized plates were exposed to the vapour of mercury. The *negative electrical* pictures were not so frequently well fixed by iodine vapour as the *positive electrical* ones. If, on the contrary, fresh plates were first exposed to the vapour of mercury at a temperature of 167° Fahr., and then had the picture produced upon them, the vapours of iodine then fixed the negative electrical image too, and that with great precision, though not unfrequently as a *positive* picture; whereas the *positive electrical* pictures were noways improved by the process. I have not been able to detect anything like regularity in

the transition of *negative electrical* pictures into positive ones, owing to the previous application of mercurial vapour, although it appears to me as though the quantity of the electricity employed was of considerable influence thereon, as is indicated by the following series of experiments.

Nature of the Electrical Picture.	Number of turns of the Machine.	Nature of the Picture after Iodisation.
positive	30	positive
positive	100	weak negative
positive	200	negative
• positive	30	positive
• positive	100	weak positive
• positive	200	indiscernible
negative	30	negative
negative	100	weak positive
negative	200	positive
• negative	30	weak positive
• negative	100	negative
* negative	200	positive

In the experiments marked with an asterisk the clean plate was exposed to the vapours of mercury at 167° Fahr. previous to the picture being formed upon it.

I am far from looking on the result as constant, or indeed from drawing any decided inference from these experiments, the amount of electricity would, however, appear to effect the nature of the picture produced, but what law this influence follows I cannot at present determine. The positive pictures that have proceeded from *negative electrical* ones, are distinguishable by their colour from those originally *positive*; the latter, upon brass, are of a blue colour, whereas the former are of a beautiful golden-yellow. The experiments here described were all performed upon brass plates, for I have not yet been successful in fixing the pictures on silver, although aqueous vapour brought them out with great precision. The metallic nature of the plate itself appears also to exert an influence on the kind of picture that is produced, but up to the present time the whole affair is too undecided to admit of any determinate opinion in the matter.

III. On reviewing all the peculiarities by which these electrical pictures are distinguished from those produced by light, we arrive at the conclusion that they are identical with those of Professor Moser produced by the invisible rays. The first thing that points out a distinction between pictures produced by electricity and those produced by light, is their conduct with regard to the fixing vapours.

The vapour of mercury does not bring out an electrical picture, whereas the vapour of iodine does. If one pursues the same process with the electrical pictures as with Daguerreotypes, no result whatever is obtained; that is to say, if one takes an iodized plate

and produces a picture thereon, the vapour of mercury will not bring it out, although the image really is on the plate, as may be shown by breathing upon it. On exposing an iodized plate with the image already on it to the rays of the sun, the picture is thereby brought out, the entire plate blackening, with the exception of the picture, which loses its colour, or becomes paler at least. In other words, it comes out as a negative picture.

Some change it is true has been thus worked in the iodine, and thus far electrical pictures and Daguerreotypes agree, the modification itself however is essentially different; with electrical pictures the effect is such as is brought about by and upon bodies of every kind, whereas in Daguerreotypes the iodine is affected in a way peculiar to itself alone. Generally speaking, it seems somewhat a remote question to enquire whether these pictures are caused by light or by heat. Supposing the pictures had been first produced by means of electricity, would it ever have then occurred to any one to attribute the effect to electrical light, or to an excitation of heat by this fluid? Assuredly not, the pictures would doubtless have been attributed to some peculiar property of electricity. This enquiry will be quite superfluous should it be established that these pictures are similar to those produced by the invisible rays. And that this is the case is evident from all the phenomena. On comparing, for instance, the properties of both kinds of pictures *die beim Nivelliren hervortreten, (?)* it will be found that they are precisely alike. Pictures produced by the invisible rays (*werden durch keine Farbe nivellirt, auch nicht durch sich selbst, (?)*) and this is also the case with electrical pictures. Blue, violet, (aqueous vapour, vapour of iodine according to Professor Moser) and the daylight or sunshine develop the pictures of the invisible rays, the effect is begun thereby, just as is the case with the electrical pictures. Red and yellow (vapour of mercury) on the other hand, do not allow of the pictures coming out, or at least only after a very long interval; this is also the case with the electrical pictures. And moreover the fact that I have stated of an electrical picture on an iodized plate not being brought out by mercury, is also in harmony herewith. In short all the phenomena bear out the notion, that the electrical pictures belong to the family of those developed by the invisible rays; but the inference that I should for that very reason draw from them would be, that these invisible rays of light were not light at all. At all events, the name selected does not agree with the peculiar character of the phenomenon; Professor Moser says himself that the invisible rays form no part of the light of the sun, that they however belong to all bodies; in this case it is not, however, light, but the operation of some power possessed by all bodies in common. The connecting link between pictures truly formed by light and those caused by the invisible rays is simply this, that both indicate an altered condition of the surfaces of the bodies, and that this

modification is more decided in the case of the pictures caused by the invisible rays than the others. The whole of the phenomena indicate modifications of cohesion; light, heat, and electricity have this one point common to them all, and I therefore am led to the conclusion that the cause is to be sought much rather in cohesion than in light. But how can one as yet touch upon the essential nature of a thing, the facts concerning which are so far from being established; a truce therefore to all hypothesis. The effect of light upon the iodized plates is similar, be the metal what it may. Although, as before stated, I was not successful in obtaining pictures on silver plates by the application of iodine in a state of vapour, yet I found that light brought out on silver and also on plates of other metals the *electrical* pictures impressed on the iodized plates. Professor Moser makes no distinction between the vapour of water and iodine according to their latent colour, that is to say, he ascribes to each the similar property either of expunging (?) *zu nivelliren* or developing the pictures. This does not altogether agree with the phenomenon as presented by the *electrical* pictures, inasmuch as the vapour of water always develops them more satisfactorily than that of iodine does, and often indeed brings out sharp pictures where iodine vapour shows not a trace of them.

IV. With galvanism I have performed experiments on the production of these pictures, but have not obtained more than equivocal results. A battery of 40 pairs of 7-inch plates produced no effect either when the current was led through a piece of money resting on glass or on a metallic plate, or when the coin formed one pole and the plate the other. Nor was the effect from a calorimotor greater, although it gave good sparks on closing the circuit. I obtained, it is true, every time, the mark of the edge of the coin, but as the operation lasted too long, I cannot well say the image thus obtained was galvanic. By magnetism, on the other hand, I obtained unequivocal effects. An iron medal resting on a steel mirror received a certain number of touches, and there was seen on breathing on the steel the edge portion of the medal. It was established by several counter experiments, that these pictures were to be attributed to no other cause. I am therefore of opinion that successful results might be expected from powerful magnets. It was not in my power to ascertain whether electro-magnetism and magneto-electricity produce similar effects, inasmuch as I had not the requisite apparatus at my command.

V. To conclude, I have produced very distinct pictures by heat, and this in several ways. A plate of glass with a coin laid upon it was placed in the warm stove, and after an interval of five minutes the plate was removed and cooled. Sometimes the image was seen on the plate being breathed upon, but what is very remarkable, occasionally there were no signs whatever of an image till the plate was cleaned off, a circumstance that I have frequently noticed with these heat pictures. Metallic plates submitted to a

similar treatment presented no certain results. When, on the contrary, a piece of money was moderately heated and a plate momentarily brought into contact with it, a beautiful image capable, of being fixed by the vapour of iodine or of mercury, was often developed.

Upon all occasions that one of these distinct pictures was produced, it was visible even without the application of iodine or mercury vapour, and was of the same bluish colour exhibited by *electrical* pictures formed on silver plates, as described under paragraph 1. I have not yet been able to establish what the conditions are under which these heat pictures are successfully produced.

[Translated by W. G. Lettsom, Esq.]

The Glacial Theory of Prof. Agassiz. By CHARLES MACLAREN.*

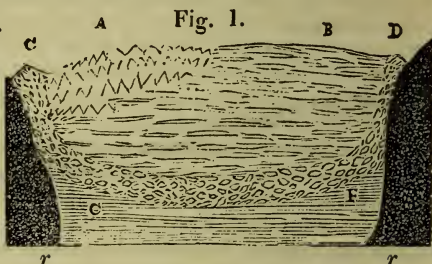
THIS is perhaps the fittest term to designate the novel opinions of M. Agassiz. Glaciers are properly long narrow masses of ice filling the bottom of alpine valleys, but M. Agassiz thinks that sheets of ice, such as are met with in Greenland, covered the whole surface of Europe, and all Northern Asia as far as the Caspian Sea. This conclusion, which has been adopted in whole or in part by Professor Buckland, Mr. Lyell, and other eminent geologists, has been deduced from a careful study of the phenomena attending glaciers, some of which are of so marked and peculiar a kind, as to afford satisfactory evidence of their ancient existence in situations where none are now seen. The Swiss philosopher advanced in his opinions step by step. He first satisfied himself that in the Alpine valleys where glaciers still exist, they once rose to a higher level, and extended farther down into the low country than they now do. Next he discovered indications of their former existence on Mount Jura and over the whole Swiss valley; and connecting these with similar indications found in the Vosges, the Scandinavian Mountains, and elsewhere, and with the well known fact of sheets of ice covering the northern shores of Siberia and entombing the remains of extinct species of animals, he came to the conclusion, that at a period, geologically speaking, very recent, all the old world north of the 35th or 36th parallel, had been enveloped in a crust of ice. Whence the cold came which produced this effect, and why it afterwards disappeared are questions he did not feel himself bound to answer, but which might, perhaps, be answered hypothetically. In reality, if we suppose the Northern Atlantic from the 39th parallel filled up and converted into dry land, it is extremely probable that Britain would have been the ice-bound climate of Labrador, with which it corresponds in latitude; and the conversion of the said land into

* This article is republished from a little tract which Mr. Maclaren had the kindness to send us, entitled "The Glacial Theory of Prof. Agassiz of Neuchatel, being an outline of facts and arguments adduced by him to prove, that a sheet of ice enveloped the northern parts of the globe at a recent geological epoch; by Charles Maclaren." As it is the best review of the subject which has met our eye, we deem no apology necessary to our readers for republishing it here.—ED.

sea would bring back the order of the seasons which we now enjoy. Even though M. Agassiz's opinions should not be fully established, they still afford us a new geological agent of great power and widely applicable, which may help us to an explanation of some phenomena very difficult to account for with our existing means of information.

Form, Magnitude and Composition of Glaciers.—The subjoined figure is not a section, but a view of a glacier as it would present itself to an eye raised considerably above it.

a b, (Fig. 1.) The glacier: *a* represents one of the forms of its surface, in which it is bristled with cones of snow or ice, called *aiguilles* or needles: *b* is the other and more usual form of the surface, consisting of narrow ridges or corrugations, like the waves fixed by frost.



c d, *Lateral Moraines*, consisting of long lines of boulders and gravel, which having been detached by frost, rain, lightning, or avalanches, from the rocks flanking the valley, settle on the two sides of the glacier. The heat reflected from the rock fuses a portion of the ice nearest it, or hastens the evaporation, rendering the sides of the glacier a little lower than the middle, and giving the mass a convex shape. The fragments rest in the hollows thus produced, and assume the form of the roof of a house, one side sloping down to the rock and the other to the ice.

e f, The *terminal moraine*, a line of boulders and gravel at the lower end of the glacier, which it pushes before it when advancing, and leaves behind it when retreating. In the latter case it looks like a low mound or barrier across the valley. The terminal moraine is a continuation of the two lateral, but they are not always found united.

r r, The rocks forming the flanks of the valley.

In the higher parts of the Alps, the perpetual snow forms vast extended masses joining the peaks and ridges, and these called *mers de glace*, or "seas of ice," exhibit scenes of grandeur and desolation which have excited the wonder of travellers. The glaciers are branches or offshoots from these, filling the valleys which descend from the higher regions to the lower. Glaciers pass down sometimes to so low a level as 3000 feet above the sea in Switzerland; but they do not originate at a lower elevation than 7000 feet, and they rarely exist on isolated mountains, whatever be their height. In the upper part they consist of granular snow, called *nevé* in the Alps which is changed into minute crystals of ice by the infiltration of water, arising from the outer portion of the snow being melted by the sun.

As we descend from the higher end of the glacier, the crystals which are rather irregular fragments generally become larger. Towards the lower end they are from half an inch to an inch and a half in diameter, and in some rare cases three inches. If a section of the glacier is exposed, the upper strata (for it is generally stratified) are found to be full of cells, and its substance becomes gradually more compact downward, the lowest part being the most solid. The strata are thick at top, thinner in the middle, and disappear towards the bottom. Glaciers contract in breadth and depth as they descend; one a league broad at the head will sometimes be only 150 or 200 yards at the foot. The thickness varies from 80 to 100 feet at the lower, and from 120 to 180 feet at the higher end. M. Agassiz adopts these measures from Hugi, and seems to reject the notion of older writers, that some glaciers are 500 or 600 feet in depth. Glaciers are of all lengths—from 100 yards to 15 miles.

Every glacier discharges a stream from a vault in its lower end in summer, which disappears in winter, except in some cases, where the water is believed to come from deep springs, with a temperature sufficiently high to keep their channels open.

There are numerous open rents or fissures (called *crevasses*) in every glacier, caused partly by the uneven surface over which the glacier glides in its downward motion—partly by the unequal expansion of the upper and under strata of ice. These fissures are of all widths—from a quarter of an inch to thirty feet or more; they are largest and most numerous at the sides, but sometimes extend completely across; they occasionally reach from top to bottom, but more frequently stop at a certain depth. Their direction is generally across the glacier, but they often become oblique at the sides, as the ice moves faster there than at the middle; and hence, viewed on the great scale, they present a curved or arched appearance, with the convexity turned towards the head of the glacier. The fissures are largest and most numerous at the lower end, and in the parts which are much inclined. In a steep valley, a glacier with its wave-like ridges, its bristling cones, and the pointed rocks piercing its surface here and there, has been aptly compared to a cataract stereotyped.

The cones or needles of ice, as at *a*, figure 1, are thus accounted for by Agassiz: The glacier, in passing along a valley whose bottom is very uneven, breaks into numerous vertical prisms and the summits of these, having their angles wasted away by the sun's heat and evaporation, gradually assume the conical shape.

Glaciers descend into regions where the annual temperature is eight or nine degrees above the freezing point; and to use the words of Cox, there are localities in Switzerland where you may almost touch growing corn with the one hand, and the ice of the glacier with the other. They of course waste away at their lower end rapidly in summer, partly by fusion, and partly by huge fragments of the ice falling off, in consequence of the upper beds expanding faster than the lower, till the outer mass loses its balance and topples down.

Motion of Glaciers.—The geological action of glaciers depends chiefly on their motion, the true cause of which has been clearly ascertained for the first time by M. Agassiz. Previous writers on the subject, including the celebrated Saussure, attributed the motion of the glacier to gravitation, or the tendency of the mass of ice to descend by its weight from the upper part of the valley to the lower. This explanation accounted very imperfectly for the phenomena, and the opinion of Agassiz, deduced from a careful attention to facts, is now almost universally adopted. He considers the motion of the glacier as the consequence of expansion, and this expansion operates chiefly in the direction in which least resistance is experienced, that is, along the valley downward, and is caused by the congelation of infiltrated water. The influence of the sun and of warm winds melts part of the upper surface, and the water so produced percolates into the spongy mass, where it is soon frozen, and in freezing expands, according to a well known law. The upper strata, imbibing more water than the lower, dilate in a greater degree, but the lower strata, in dilating, carry the upper with them, and thus produce rents or *crevasses*. Again, the flanks of the glacier imbibe more water than the middle, and by their greater expansion give a curved form to the *erevasses*; and the lower end imbibes more water than the upper, in consequence of the more frequent rains and alternations of frost and thaw. Besides, as the upper end of the glacier, in expanding, pushes the rest before it, the accumulated effect of the whole expansion falls upon the lower end, which is found to travel quickest. The motion, too, is most rapid in summer, and nearly ceases in winter, in consequence of the water being then constantly frozen. From the effect of this internal movement of its parts, the glacier creeps along slowly but surely. In 1827, M. Hugi constructed a hut on the glacier of the Aar, at the foot of a fixed rock called *Im Abschwung*. It was found that the hut had receded 2200 feet from the fixed rock in 1836, and 4400 in 1840, showing that it had advanced about 250 feet per annum in the first nine years, and 550 in the four last. Taking summer and winter together, its motion had been about eight inches per day in the first period, and eighteen inches in the second. In glaciers which are much inclined, the motion is more rapid than this.

Polished and Grooved Surface of Rocks.—The glacier in its course downward carries with it the fragments of rock, gravel, and sand, which lie under it. These adhere to the ice, or are imbedded in it, and as the mass glides slowly along, they abrade, groove, and polish the rock, and the larger masses are reciprocally grooved and polished by the rock on their lower sides. The effects of this abrasion on the bottom of the valleys may be conceived from the pressure applied. A cubic yard of sandstone weighs two tons, and if we assume the average density of glacier ice to be two-thirds of that of common river ice, the pressure upon each square yard of rock at the bottom of a glacier 100 feet deep, will be equal to about sixteen tons, or the general pressure will be as great as would be produced

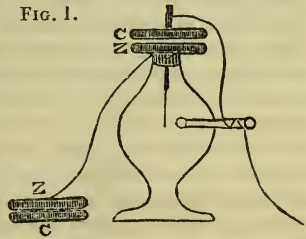
by a bed of sandstone twenty-four feet thick. Thus the various materials under the ice are pressed against the rock with an enormous force, while an equally great force of another kind, produced by the congelation of water, propels them downwards. The sand, acting like emery, polishes the surface; the pebbles, like coarse gravers, scratch and furrow it; and the large stones scoop out grooves in it. Portions of these substances, and of the rock too, are ground to a state of fine clay, and the whole of the moveable matter, stones, pebbles, sand, and clay, are in course of time thrown out at the lower end of the glacier, where they form the terminal moraine.

(To be continued, with five illustrative Engravings.)

MISCELLANEOUS.

APPARATUS NEVER BEFORE PUBLISHED.—Figure 1 is a representation of a single gold-leaf electroscope, and an arrangement for showing the mode of detecting a development of electrical action by a disc of copper, and another of zinc, whilst in close contact with each other.

The electroscope consists of a glass vessel with a globular body, having a perforation on one side, for the introduction of a moveable wire, with a ball at each end. The neck of the vessel is covered with a wooden cap, through the centre of which passes a brass wire, which is tapped at the upper end; and to its lower end is attached a pendant strip of leaf gold. The side wire being also tapped and working in a screw socket, (not shown in the figure,) permits the ball at its inner extremity being adjusted to any required distance from the gold leaf. To the top of the axial wire is screwed a disc of zinc, on which is placed a disc of copper. These discs are not in contact with each other, for the copper rests on three or four spots of sealing wax, which has been previously dropped on the upper surface of the zinc. These two discs, thus separated, form a *condenser*; the lower being the insulated plate, and the upper in connection with the side wire, and the ground, by means of a thin copper wire. The zinc and copper discs which are the objects of the experiment, are placed on the table, one upon the other, in close contact, and the zinc uppermost, having a connection with the zinc of the condenser, by means of a zinc wire, as shown in the figure. When the plates have lain in contact for a few seconds, the loose wire which rests on the zinc, is to be suddenly removed by striking it with a dry glass rod. The copper plate of the condenser being now removed, by taking hold of the wire handle, gives an opportunity for a portion of the electric fluid received by the zinc, from the zinc of the connected pair, to spread over the gold leaf, which will strike the side ball if



sufficiently close ; but if not, it will lean towards it, and by proper tests it is discovered to be electro-positive. When the electricity of the copper disc of the pair is to be examined, the whole of the plates are to be reversed, and a copper connecting wire instead of the zinc one, is to be employed. The copper shows electro-negation.

FIG. 2.

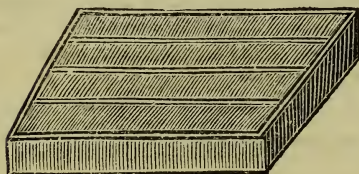


Figure 2 represents a dry electric battery, similar to an electric column, consisting of six hundred pieces of zinc, with intervening paper. Each piece of zinc is a square of about half an inch per side. One surface is made quite bright by means of a fine file,

the other is oxidated as it was found, after being some years in the capacity of water spouting at the Military College, at Addiscombe. The metals stand on edge in four rows or groups, in a glass-lined mahogany box. The rows are separated from one another by glass partitions, but their ends are properly united by wire, to bring the whole into one continuous series. The *positive* and *negative* poles are at the *bright* and *dull* ends respectively.

FIG. 3.

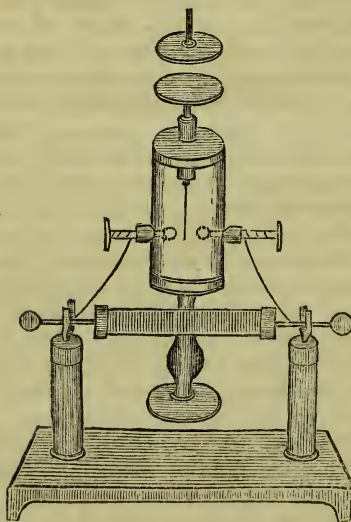


Fig. 3 represents a single gold-leaf electroscope connected with a dry electric pile. The original instrument, which was made in the year 1825, consisted of a neckless glass phial, perforated on its two opposite sides for the purpose of introducing two brass wires, which worked, by means of screw motions, in wooden caps which were firmly cemented to the glass directly over the perforations. The phial stood on a neat boxwood pedestal, as shown in the figure. The top of the glass was covered with a wooden cap, through the centre of which passed a glass tube furnished with an axial brass wire, with a narrow strip

of leaf gold suspended from its lower end. The electric pile was supported by two glass pillars fixed in a wooden base-board, and connected with the moveable lateral wires of the electroscope, by copper wires. When the inner balls were nicely adjusted, the pendant leaf would hang vertically between them, and the electric cha-

racter of any body brought into contact with the horizontal plate at the top of the instrument, would be shown by the leaf striking the polarized ball of the opposite kind. When the fixed horizontal plate was zinc, and the upper one copper, although not the size of a sixpence each, the electricity developed by one contact only, would cause the leaf to strike the negative lateral ball. And the contrary when the discs were reversed. For further particulars, see my "Lectures on Galvanism."—W. STURGEON.

Production of Sal-Ammoniac and Purification of Gas for Illumination. By Mr. FREDERIC THEODORE PHILIPPI, Calico Printer, Belfield Hall, Lancashire.

(Patented Jan. 1842.) Extract from Specification.

AMMONIA is found in coal gas combined with hydro-sulphuric acid, carbonic acid, sulphurous acid, hydro-cyanic acid, hydro-sulpho-cyanic acid, &c. When the gas is made to traverse saline metallic solutions such as the manganese, iron, zinc, copper, lead, antimony, &c., a double decomposition takes place, and a sulphuret, carbonite, sulphite, cyanuret, &c., will be precipitated, and the liberated ammonia will be held in solution. On account of cheapness, the sulphate or chloride of manganese is preferred. The solution is to be rendered neutral by using the *condensed* ammoniacal liquor of the gas works, and by this means reduced to the strength of 20° Baume. The apparatus for this process, consists of three wrought iron cisterns or wash-boilers, uniformly elevated above each other. In these the solution is placed, and the gas becomes washed by traversing the three portions. The lids of the cylindrical cisterns are to fit water-tight.

The pipes for admitting the gas into the solutions descend to about two inches below the surface, and terminate in gratings or perforated plates to allow of the greatest possible surface to be exposed to the liquid. An agitator is in each cistern for the purpose of stirring up the deposit when it is to be emptied. An escape-pipe is also attached to each cistern to conduct the overflowing liquid to the lowest in the series, in case of an increase of pressure. When the liquid in the first cistern is saturated with ammonia, which is discovered by testing, it is drawn off, and that in the second transferred to the first, and that in the third to the second: and a fresh supply is given to the third.

The saturated solution is received in a vat, and when separated from the deposit, it is transferred to another vessel and acted upon by a little sulphuric, hydro-chloric, or other acid, according to the nature of the salt which saturates the ammonia; it is then evaporated for crystallization. The process is applicable, also, for removing ammoniacal substances, which are disengaged during the distillation of bones, or other animal substances, and all such as produce ammonia by heat. Naptha, or naphthaline, which the coal

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yields, has its natural volatility increased by the pressure of ammonia. When the ammonia is absorbed, their affinity is destroyed and the naphtha or naphthaline floats on the surface of the liquid, or else remains in the precipitate. In this process the gas should be previously cooled, and the ammoniacal water condensed.

Fig. 1.

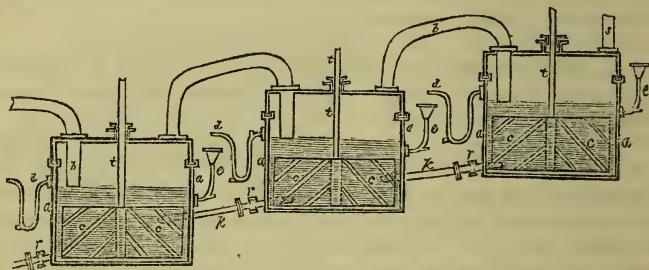
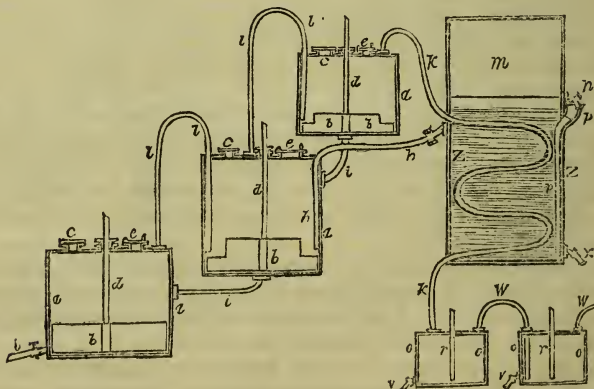


Fig. 1 is a sectional elevation of the apparatus for purifying gas, where *a, a, a*, are the cylindric wash cisterns; *b, b, b*, the pipes which admit of the gas; *c, c, c*, the agitators provided with arms; *t, t, t*, the shafts work in stuffing boxes, by means of which they can be cleaned without loss of gas, and without stopping the apparatus; *e, e, e*, are pipes with funnels for receiving the liquid which runs from the overflow pipes, or from the pump for supplying the apparatus; *r, r, r*, are taps placed in the pipes *k, k*, for the purpose of letting off the liquid; *s* is a pipe by which the gas is drawn off, and *k, k, k*, are pipes by which the cisterns are emptied from one another.

Fig. 2.



There are some instructions given for distilling the ammoniacal liquid, and the apparatus is represented by Fig. 2, which is a sectional

elevation ; *a, a, a*, are sheet iron boilers with flat bottoms, the first of which is placed over a furnace, or it may be heated by steam. The second boiler is heated by the remainder of the flames and the smoke ; *b, b, b*, are agitators, which may be constructed with rakes, to keep the lime in motion and prevent sediment ; *c, c, c*, the man-holes, *d, d, d*, the shafts of the agitators, piping through stuffing boxes ; *e, e, e*, lids supported by screws for the purpose of admitting lime into the boiler ; *l, l, l*, leaden pipes for conducting the vapours from the lower to the upper boilers ; *i, i, i*, pipes for emptying the boilers, these are furnished with valves ; *k*, a worm in the refrigerator *z, z*, which is hermetically closed ; it conducts the heated vapours from the ammoniacal liquids to the acid ; *m*, the reservoir for the ammoniacal liquids, from which they are conducted to the bottom of the refrigerator by the tap *n*, and the funnel pipe *p* ; *h*, a pipe with a tap conducting the hot liquors into the boiler *a* ; *o*, a leaden vessel with a tap at *v*, placed a little above the level of the bottom, and of the escape pipe *r*. This vessel, in which is deposited the condensed ammonia, is in communication with the vessel *o, o*, by means of the pipes *w*, forming altogether a "Woolf's Apparatus" in which the ammoniacal gas that escapes is dissolved ; *x*, a tap for drawing off the liquid contained in the refrigerator. (*Rep. of Arts.*)

Specification by Newton and Son.

MOSER'S IMAGES.—The following is the substance of a letter by M. Fizeau to M. Arago. M. Fizeau does not agree with Moser that these images are due to any "latent light." He considers them foreign to every species of radiation, and ascribes them to the *well proved existence of fatty and volatile matters* which soil the surfaces of most bodies !!! Fizeau sums up in the following manner.

1. The property of forming images on a polished surface is not permanent in bodies. For, by repeating the process on the same body, the image-forming power weakens, and after a certain number of impressions, becomes extinct. Compact bodies such as metals lose this property rapidly, whilst porous bodies retain it in a remarkable manner.

2. When this property has become extinguished in any body, it can be restored by passing the fingers along its surface ; or by rubbing with the hairs of a living animal, which always contains grease.

3. If, without altering the temperature of the polished surface which is to *receive* the image, the temperature of that from which it is to be taken be raised, the image is formed in a short time.

4. The surface which has *received* the image will communicate another image (secondary) to a second plate, and this again would give a third image, if the perfection of the impression did not diminish very rapidly by these successive transfers.

5. When a thin film of mica is interposed between the body *giving* the image, and the polished surface, the failures are frequent, though under certain circumstances images may be obtained, which it is of importance not to confound with those that the body itself would

have produced. For instance: If the same film of mica be used for two successive trials, and that in the second trial its position be reversed, the surface which was in contact with the body in the first trial, having received an image, now transfers it to the polished surface^e against which it is placed, or forms on that surface a *secondary* image. This image may always be distinguished from the direct image, because it will be formed in the reverse order with respect to the right and left sides.

6. Lastly, The results are the same whether the experiments be made in the light, or in total darkness.—*Comptes Rendus*.

Extract from a Letter of M. Knorr to M. Bregnett.

"I have been engaged for four weeks in following up the discoveries of Moser, upon "invisible light." Without entering into theoretical speculations, I believe the facts which I have obtained sufficiently prove that all the actions that Moser attributes to *invisible light* are due to heat alone.* I have created an art entirely new, which I have named *thermography*;† for I have found that visible images can be formed without any condensation of vapour on the plates, by the mere action of heat. There are three different methods. 1. Images may be formed in from 8 to 15 seconds, but not with constant success. 2. Is applicable to bodies which are not very good conductors of heat. 3. This deserves preference, as it answers better and with almost constant success; but it requires from 8 to 10 minutes to obtain a good image. By this method I have obtained proofs of coins of platinum, gold, silver, engraved plates of copper, brass, and engraved stones, steel, and glass; also of prints. The images were formed on plates of silvered copper, pure copper, steel, and brass."—*Ibid*.

DAGUERRETYPE BY GALVANIC LIGHT.—By some experiments which have been made by Mr. Silliman, A.M., and Dr. W. H. Goode, at Yale College, U. S., with a galvanic battery of 900 pairs of 10 in. by 4 in. plates; and a camera obscura, with an achromatic lens, they have obtained Daguerreotypes from a medallion. The light was that from charcoal points, which also gave images on the plate. The colours of the two images of the charcoal points differed remarkably from one another: one was of the colour of the flame afforded by the combustion of an alcoholic solution of strontia; the other resembled the flame produced by the combustion of an alcoholic solution of chloride of sodium. Several charcoal points were tried, and frequently shifted from one pole to the other, but the effect was still the same, the difference of colour being due to the poles of the battery, each of which invariably produced its peculiar coloured image. The experimenters thought "that the yellow-coloured image was produced from the charcoal point in connexion with the positive pole of the battery," and, consequently, the fiery-coloured image from the negative pole.—*American Journal of Science*.

* This view is in accordance with that which we have entertained from our own experiments. See *Annals of Electricity*.—EDIT.

† This term was first employed by MR. HUNT.—"Annals of Electricity," vol. x.—EDIT.

THUNDER STORMS.—On Thursday, June 8th, a heavy thunder storm passed over a great part of Lancashire and Cheshire. In the morning the lightning and thunder were accompanied by heavy showers of rain, at Blackburn. At Wigan the principal part of of the storm was about mid-day, where there were lightning, thunder, and heavy hail showers. At Standish Lower Grounds, a little more than two miles from Wigan, an oak tree was struck by the electric fluid, and a portion of it rent away. One of the top branches was struck off, and the lightning entered the trunk, and tore away the bark, and a shred of timber about ten yards long, loosening the bark round the whole of the trunk, and making a large hole in the earth close to the root of the tree. Six men were standing for shelter a few yards from the tree at the time it was struck, but they sustained no injury. We think them very fortunate in escaping this terrible element; and would warn all persons to keep clear of tall trees during the appearance of lightning. More deaths have occurred, by lightning, in those situations, than in any other.

The same thunder storm raged nearly the whole day about Manchester, but we have not heard of any injury being done. A tall factory chimney, near Bolton, has been struck by lightning twice within the present year. It was completely shattered the first time; it was patched up again, and it was struck the second time last month. It is now again being repaired; but no lightning conductor is, even yet, thought necessary by the proprietor!!!

SINGULAR DISCOVERY.—On Saturday morning, the 3rd inst., as three labourers employed on the estate of Mr. P. Flemming, of Harewood, near Bricknall, were excavating the ground within the precincts of an extensive wood, called the “Fells,” for the purpose of laying down pipes to convey water to a reservoir in the park, they discovered a descent, formed by several steps cut in the earth, at the termination of which there appeared an opening, sufficiently large to admit of a person passing through. One of the men explored the opening for some yards; but feeling somewhat timid, and neither of his companions being inclined in venture into the passage, they refrained from proceeding; and on going down to the lodge at the dinner-hour, they mentioned the circumstance to Mr. Stains, the house steward, who accompanied the men to the spot, taking with him a small lamp. On advancing a few yards, he emerged into rather an extensive cavern, formed apparently by nature in the side of a high bank, partly composed of rock, through several narrow fissures, in which the daylight was dimly admitted. On examining the interior he discovered the remains of a human body, consisting of the skull and principal bones, several of which had become detached, from the length of time they had lain there. A pair of old-fashioned steel spurs, completely encrusted with rust, and which, from their position, had evidently been attached to the feet of the deceased, although not the slightest vestige of either boots or straps remained, were found, together with the barrels and locks of

a pair of pistols, the stocks of which had long since rotted off. Several large buckles, and a number of what appear to be steel buttons, which might probably have been attached to the dress of the defunct, were likewise found mingled amongst the bones.

ROYAL COMMISSION.—STATE OF LARGE TOWNS, &c.—WHITE-HALL, MAY 9th, 1843 :—The Queen has been pleased to direct letters patent to be passed under the great seal of the United Kingdom of Great Britain and Ireland, appointing Walter Francis, Duke of Buccleuch and Queensbury, K.G.; The Right Hon. Henry Pelham Clinton, (commonly called Earl of Lincoln;) Robert Aglionby Stracy, Esq.; George Graham, Esq.; Sir Henry Thomas de la Beche, Knt.; Dr. Lyon Playfair; Dr. David Boswell Reid; Professor Richard Owen; Capt. William Thomas Denison; James Ranald Martin, Esq.; James Smith of Deanston, Esq.; Robert Stephenson, jun. Esq.; and William Cubitt, Esq.; to be Her Majesty's Commissioners for enquiring into the present state of large towns, and populous districts, in England and Wales, with reference to the causes of disease among the inhabitants.—The Queen has also been pleased to appoint Henry Hobhouse, Esq. to be Secretary to the said commission.—*London Gazette, May 9th, 1843.*

THE SCREW STEAMER.—This extraordinary little vessel continues to excite great curiosity and surprise on the river, twisting and turning as she does among the numerous craft off George's pier with a perfectly snake-like power and facility. A few days ago her capabilities in towing were tested, and with as much success as the previous experiments on her speed, which is known to be superior to any of the ferry boats on the river. Three flats, each containing fifty tons, were attached to her and taken up to Runcorn in two hours and eight minutes, against a strong breeze from the S.W. which is fully equal to what is usually done by steamers of fifty horses power: and, subsequently she drew three heavily laden barges, on the Old Quay Canal, at the rate of three and a half miles an hour: whereas it takes three horses to tow one barge only two miles an hour, in the same canal. The consumption of fuel in the *Screw*, is only one and a quarter cwt. per hour. The propelling power in this steamer, although placed in the stern like the *Archimedes*, is not the same as in that well known vessel, but assimilates very much to the sweeps of a windmill; and, as far as our means of comparison have gone, we should consider it the more efficient of the two, and vastly superior in many important points, to the old paddle wheel plan. We advise all lovers of mechanical ingenuity, to examine this singular little steamer.—*Gore's Liverpool Advertiser.*

THE ANNALS
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PHILOSOPHICAL DISCOVERY,
AND
MONTHLY REPORTER
OF THE
PROGRESS OF SCIENCE AND ART.

NO. 2.

AUGUST, 1843.

The Glacial Theory of Prof. Agassiz. By CHARLES MACLAREN.

(Continued from page 45, and concluded.)

The ice, in consequence of its tendency to dilate, and its numerous fissures, accommodates itself to the sinuosities of the rocks which confine it, cutting off the smaller projections, and rounding and polishing the larger, which assume the form of domes, and were termed *roches moutonnées* by Saussure. Agassiz's eighth plate gives some fine examples of these rounded swells. Owing to the immense pressure, the included pebbles of conglomerates, and the hardest veins in veined rocks, are cut away to the very same level with the softer parts which envelop them.

Thus, one of the marks by which the ancient existence of glaciers can be detected in situations where they are no longer seen, is the polished, striated, or grooved appearance of the rocks. Sometimes it is very distinct, but in many cases it is not visible, because the surface of most rocks wastes away by disintegration or decomposition, unless it is well protected by a covering of clay or turf. The most satisfactory specimen near Edinburgh, is in the quarry on the south side of Blackford Hill, at a place laid open a few years ago, where the rock leans forward, forming a sort of vault. The surface of the clinkstone here, for a space of ten or twelve feet in length, is smoothed, and marked by *striæ* or scratches in a direction approximating to horizontal. We accompanied M. Agassiz to the spot about two months ago; he had expressed doubts as to some other supposed marks of glacial action near this city, but on seeing those at Blackford Quarry, he instantly exclaimed—"That is the work of the ice." On the top of Salisbury Crags, at a quarry about two hundred yards from the south extremity, the polishing is very well seen at intervals over a space of twelve or fifteen feet just at the edge of the precipice; and *striæ*, running east and west, will also be discovered here by an eye accustomed to observe them, though they are much less distinct than at Blackford Hill. In quarrying
Annals of Philosophical Discovery, &c., No. 2, Aug. 1843. F

the Crag at this spot, the rock had been cut back about 120 feet from what was originally the edge of the precipice, and this part, which had been well protected by the turf, was only exposed about 1822 or 1823. We have little doubt that similar appearances would be presented if other parts of the greenstone, equally distant from the edge of the precipice, were *newly* laid bare. . . Parts of the north end of the Castle-rock are also curiously polished, and the groovings on the western slope of Corstorphine Hill, described many years ago by Sir James Hall, are well known. We have observed similar marks of abrasion at Craigmillar Quarry, Craigmillar Hill, and elsewhere.

These marks of abrasion, both on rocks *in situ*, and on boulders found in the soil, have been usually attributed to the action of the currents of water, rolling along stones and gravel, an explanation felt not to be satisfactory, but adopted for want of a better. It is admitted that rocks in the channels of rivers are often worn smooth; but Dr. Buckland contends, and apparently on good grounds, that straight parallel *striæ* and grooves never are, and cannot possibly be, produced by the action of gravel and stones in a stream. The abrading material, say a fragment of rock, if it rolls along, will perhaps make occasional indentations, or now and then an irregular scratch; but it cannot produce straight, parallel, continued *striæ* or grooves, unless held fast by some substance which prevents it from rolling, and gives its motion a determinate direction, as the cutter in a grooving plane is kept in a fixed position by the wood. Now, the ice of a glacier (or iceberg) is an agent which answers this purpose admirably; we see that it actually produces the effects described; we know no other agent capable of producing them; and it is therefore inferred that where well defined *striæ* or grooves are found on rocks, we have evidence of the former existence of moving masses of ice.

Moraines.—These afford other evidence of the ancient existence of glaciers after they have disappeared. Long terraces or banks of gravel are occasionally found on rocks forming the sides of valleys, high above the bottom, and where the surface they rest on is much inclined. Geologists have felt the difficulty of accounting for these deposits. Their situation is inconsistent with the idea that they were formed by running water; neither could they be deposited on the margin of lakes, because their parts are often found not to be on the same level. Again, they are found stretching like bars across the mouths of valleys, in situations from which a great current, so far from depositing them, would have swept them away, if they had previously existed. Now, both kinds are well accounted for on the supposition that they were the *moraines* of glaciers; those on the sides of the valley being *lateral*, and those barring up its mouth being *terminal* moraines.

Again, we sometimes find one or two long ridges of gravel stretching through a wide valley lengthwise or obliquely, without dis-

covering any thing in the shape of the valley to indicate why the current, if water was the agent, should have accumulated the moveable matter here, rather than spread it over the surface. This also is explained, if we assume that it was a *medial* moraine. When one valley opens into another, the two lateral moraines on the inner sides unite, and the compound glacier, besides having a line of blocks and gravel on each side, has a third stretching along the middle, and which is therefore called a *medial* moraine. There are examples in Switzerland of glaciers with three, four, or six medial moraines. Now, were the glacier to melt away, owing to a change of climate, these three, four, or six medial moraines would form as many ridges of gravel running along the bottom of the valley, or obliquely through it, and would resemble deposits occasionally seen in this country.

When a compound glacier is long, the different moraines, lateral and medial, sometimes become blended in their progress downwards, and spread out into a broad sheet; and if the ice were to disappear, we should find the whole bottom of the valley at this part covered with a confused assemblage of fragments of rock. This is also a deposit occasionally met with in Scotland.

The materials of moraines are not stratified, but huddled together in confusion. The fragments are generally somewhat rounded by mutual attrition, but some are angular. They may be distinguished from the banks of gravel formed at the margin of lakes by their internal structure, by the difference of level between their distant parts, and also by their form.

We are not quite sure of the precise shape of *terminal* moraines, but the terms employed by Agassiz (*diques ou remparts*) lead us to suppose that they form long mounds with rounded sides. Like the others, they are not stratified internally; but, from the manner of their formation, they contain more finely triturated matter, namely, clay, sand, and small gravel. Agassiz seldom gives precise measurements; but he mentions one terminal moraine, (that of Viesch,) which is thirty feet high, and much more in breadth. Glaciers sometimes advance for a term of years, and then retreat for another term. When a glacier is retreating, it forms a new terminal moraine every year, and when it again advances, it pushes the more recent ones before it till the whole are blended into one mass. Now, if the disappearance of the glaciers took place gradually, as it seems most reasonable to suppose, we ought to find in the lower end of some of our valleys a series of little transverse mounds, like *x*, *y*, in figure 5, below.

Lateral moraines increase in size towards the lower end of the valley, and for an obvious reason: The fragments which fall at the head of the valley are slowly carried downwards by the glacier in its course, and they are joined in their progress by those which fall from the rocks in the lower part of the valley. Blocks which fall into the *névé* or granular snow high up, sink into it and disappear

for a time ; but it is curious, that except those which tumble into *crevasses* and reach the bottom, they all afterwards rise to the surface. Agassiz thinks, that the internal dilatation which makes the glacier travel downwards, also operates upwards, and carries all included masses to the surface. It is certain that an enclosed boulder is never seen in the terminal section of a glacier, where the composition of the mass can be best observed. In consequence also of the sides of the glacier travelling faster than the middle, and of its breadth generally diminishing towards its lower end, it very often happens that the blocks of medial moraines find their way to the sides and join the lateral ones.

The ascent of blocks from the middle or lower part of the ice to the surface, explains another curious fact—that though the general motion of the glacier is along an inclined plane downwards, scratches are often found on the rock inclined in the opposite direction ; that is to say, supposing the surface of the glacier to dip at 10 degrees to the north, you will find scratches dipping at 10 or 20 degrees to the south, or even vertical. These are caused, in Agassiz's opinion, sometimes by inequalities in the bottom of the valley, but frequently by enclosed blocks working their way upward by the expansion of the ice, while the glacier is travelling downwards.

FIG. 2.

FIG. 3.

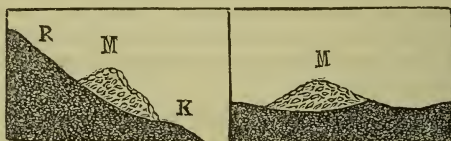
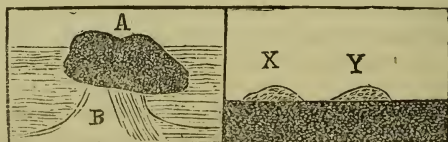


Figure 2 represents the usual form of a lateral moraine in the cross section, and as it would appear on a surface considerably inclined ; M the mass of gravel forming the moraine ; R K the rock on which it rests. Figure 3 represents the form which, it is assumed, a compact medial moraine would have if the ice were melted, and the matter left on the surface of the valley.

Retreating glaciers form a terminal moraine every year, as mentioned in page 59 ; and in this case we might expect to find a series of mounds transverse to the valley, like x y, figure 5.

FIG. 4.

FIG. 5.



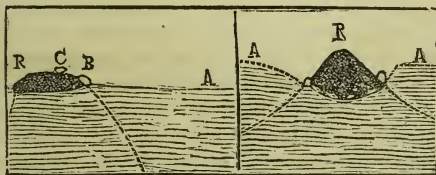
Erratic Blocks.—Single blocks of huge size are often seen resting on the surface of the glacier, and travelling downwards with it. These are generally angular, and they often stand on pedestals of ice, as in figure 4, where A is a tabular mass of rock, and B the

pedestal of ice.* Agassiz describes one he saw on a glacier, which measured 20 feet by 12, and must have weighed 100 tons or more. In accounting for the pedestal B, he observes that gravel, when it rests on the surface of a glacier, being heated through and through by the sun's rays, melts the ice below it, and gradually forms a pool or well in it. A large block, on the other hand, has only its upper surface heated, while the inferior mass, remaining cold, protects the ice below—both from the action of the sun's rays, and from the evaporation by which ice, like water, wastes away in the open air, and thus, while small stones often sink into cavities, large ones seem hoisted on pedestals. Masses of all kinds tend towards the sides of the glacier, and many of these huge blocks are found scattered along the flanks of the Alpine valleys, some having remained there, stranded as it were. Others are found in the middle, far from existing ice, and were probably left there when the glacier disappeared. We have thus an explanation of the erratic blocks so common in this country, when these do not come from very distant stations. Being stranded by their greater weight, while the smaller matter moved onward, or left sticking on the soil in consequence of the final fusion of the ice, we can understand why they are often found *perched* on the sides of steep declivities.

Blocs perchés, so named for the reason just given, are sometimes found in very singular situations.

FIG. 6.

FIG. 7.



Let A, figure 6, be the surface of the glacier, R the top of a projecting rock *in situ*. The ice has the block B floating on it; it encompasses the fixed rock nearly on a level with its summit, and in travelling downward strands the block upon it. The block may be stranded on the very summit, as C. Supposing the glacier afterwards to disappear, here we would have an angular block perched on an isolated hill, or as Agassiz terms it, a pyramid, with a steep declivity below it, and we would be puzzled to conceive by what agent it was planted in so singular a situation.

Figure 7 represents erratic blocks in a very different situation, but quite as singular. R is a projecting fixed rock, rising considerably above the glacier A A; the reflection of the sun's heat from its surface melts a portion of the ice, and forms a cup-shaped cavity round it. Into this cavity blocks of various sizes fall by their weight from the surface of the ice as it glides onward, and settle on the

* This figure is borrowed from Agassiz's fourteenth plate. Figure three, and all the others, are ideal, and are suggested by his descriptions.

flanks of the hillock. Supposing the glacier to disappear, this conical rock would have a ring of stones like a coronet encircling its summit, and we would be apt to wonder at the mysterious agency which brought them there, and left the lower parts of the hill destitute of them. Agassiz names various isolated rocks amidst the Alpine glaciers with such circles of stones round them, or with single blocks stuck upon them, as in figure 6. The same phenomena reappear on Mount Jura, where no glaciers now exist.

Creux and Lapiaz.—On the sides of the Swiss valleys, round holes, such as cascades make, are sometimes found in the rock; but in places remote from running waters, and where the form of the surface will not permit us to suppose that any cascade could ever have existed. In other cases, a long, sinuous, dry, water-worn gutter or channel is observed, the course of which runs *across*, instead of *along*, the natural declivity of the ground. The study of the glaciers has enabled Agassiz to find a key to these enigmatical phenomena, which had perplexed previous inquirers. Streams of water flow along the surface of a glacier, and when one of these falls into a fissure which is open to the bottom, it often forms a cascade, and cuts a round cavity in the rock with the gravel and sand which it finds there, or carries down with it, as some of our rivulets work out the hollows termed cauldrons. When no fissure exists, the stream sometimes cuts a funnel or shaft (*couloir*, *entonnoir*) through the ice by the action of gravel. If the glacier is travelling downwards, the cascade will travel with it, and convert the round cavity in the rock into a long gutter; or, supposing the water to reach the bottom without falling in a cascade, still, in finding an issue below the glacier, it will be compelled to follow the sinuous openings left by inequalities in the bottom of the ice, and thus take a course at variance with the natural inclination of the surface. We have here an explanation of the *creux*, or holes, and the long water-worn gutters found in such unlikely situations, which bear the local names of *lapiaz* or *karren*. These are chiefly observed where the rock is soft, and are seldom visible on the granite.

Stratified Gravel on sides of Valleys.—When a small portion of stratified gravel or sand is found adhering to the side of a valley, high above its bottom, the conclusion usually come to is, that a lake or arm of the sea had once filled the whole up to that level, and that the deposit is merely a remnant of one much more extensive. Agassiz has shown that this conclusion may be erroneous. When the streamlets flowing on or under a glacier, cannot find an escape below, they often form small lakes at the surface on its flanks, and, as in other lakes, the gravel and sand carried into these, arrange themselves in strata. This stratified deposit may be continuous with, and form as it were a portion of, a lateral moraine, which is not only unstratified, but which follows a line probably far from level. Here again the study of existing glaciers enables us to explain very anomalous appearances.

Glacier Barriers.—A glacier descending a valley opening into another, sometimes pushes forward till it forms a dike or barrier across the latter. Behind this the water collects and constitutes a lake, which augments till it breaks the icy barrier, or flows over it, producing frightful inundations. In 1815 the glacier of Gétroz formed a dike across the valley of Bagnes. This dike went on increasing till 1818, when it was 500 feet high and 800 long. It was then burst by the pressure of the waters of the Drance, which committed terrible ravages as far down as Martigny. The lake of Distel, on the Saas, those of Rufnen and Gurglen, in the Tyrol, and that of Passey, on the Adige, are also formed by glaciers. The last has burst its dike six times with the most destructive effects, since 1404. Agassiz traced decided marks of an ancient glacier at the north side of Ben Nevis. This glacier, he thinks, had closed up the valley of the Spean, and formed a lake in Glenroy, in which the banks of gravel, called Parallel Roads, were deposited. The barrier being of ice, which subsequently melted, the absence of any marks of its existence is accounted for. At present we shall not stop to inquire whether this theory or Mr. Darwin's is the more probable.

Alluvial Deposits.—Agassiz thinks that the floods produced by the bursting of such lakes as those described, and by the fusion of the ice, tore up the moraines, scattered their materials over the country, and formed the unstratified boulder clay, and the stratified sand and gravel resting upon it, which now cover nearly the whole surface of the low country.

Ancient extent of Glaciers in Switzerland.—The traces of ancient lateral moraines are seldom very distinct; yet in the lower valleys, where no glaciers now exist, in that of the Rhone, for instance, between Martigny and the lake of Geneva, several may be seen ranged in parallel lines, one above another, at 1000, 1200, and even 1500 feet above the river. Terminal moraines are found half a mile, a mile, a league, and even several leagues from existing glaciers; but these are in secondary valleys, and belong to the period when the glaciers were retreating into the narrow limits which they now occupy, while the floods which occurred at this period had obliterated those of the principal valleys. The striated and polished surfaces, which had a more durable existence, are found at great heights; among other examples, on Seidelhorn, (an isolated mountain in the Alps, now destitute of glaciers,) 2590 feet above the bottom of the valley, indicating that ancient glaciers of this depth or more existed here. The boulders also, or *blocs perchés*, the *creux* or pits, and the *lapiæz* or water-worn gutters, were all observed far beyond the present limits of the glaciers. This first step in the argument conducts Agassiz to the conclusion that *the whole of the Alps, at some ancient period, formed one vast mer de glace*, the ice descending to the level of the great Swiss valley which separates these mountains from Jura.

But the same indications of glacial action exist on Mount Jura, which runs parallel to the Alps, divided from them by the great

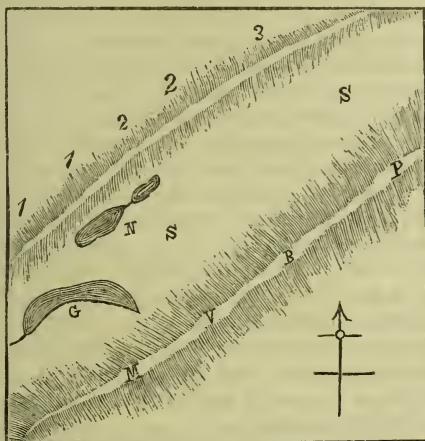
Swiss valley, fifty miles in breadth. This chain, which is of moderate height, is now entirely destitute of glaciers, and, owing to the nature of the rock, the marks of abrasion are remarkably numerous and distinct. They are found *on the side fronting the Alps* from the bottom to the summit, and from Ecluse, near Geneva, to Aarau, a distance of 130 miles. (*Sur tout le versant meridional de Jura, depuis le Fort de l'Ecluse jusqu'aux environs d'Aarau.*) When the surface is newly exposed, it is smooth as a mirror, marked with furrows and fine scratches, and exhibits the *roches moutonnées*, or rounded undulations and domes. But the most characteristic fact is, that *the furrows do not run from the summit downward, but in a horizontal or oblique direction along the face of the ridge*, showing that they were impressed by a body moving parallel to the chain along its southern flank. In form and position, they are, in short, precisely similar to the furrows produced by existing glaciers on the sides of the valleys *along* which they move. Further, these polished and striated rocks are not confined to the declivities of Jura, but are found equally at their foot, in the bottom of the great Swiss valley, wherever the rock is calcareous.

In addition to these striated and polished surfaces, Jura has its moraines, and in these moraines patches of stratified deposits are found, such as are now formed in small lakes on the flanks of glaciers. It has thousands of erratic blocks, distinctly derived from the Alps; and, that nothing might be wanting to complete the chain of evidence, Jura has its *lapiaz*, or water-worn gutters where no water runs; its *creux*, or water-worn pits, in situations not dominated by any rock whence a cascade could fall; and its salient peaks, surrounded by coronets of boulders, as in figure 7. Now, as no ridge occurs between the Alps and Jura, it is evident that the mass of ice which pressed against the southern declivities of the latter to the height of 3500 feet or more, with a force sufficient to cut and groove the surface longitudinally, must have extended far into the great valley or low country; and as striated rocks and travelled boulders are also found all over the bottom of that valley, and on the Alps at its opposite side, we have before us a concatenated series of facts, leading almost inevitably to the conclusion that a *mer de glace*, or vast sheet of ice, once enveloped the Alps and Mount Jura, and covered the whole of the low country between them. Hemmed in by the two mountain chains, the ice could expand only in a northeast or southwest direction, and Agassiz infers from the direction of the *striae*, that in the middle and northern part of the valley the motion was northeastwards, or towards the lake of Constance.

Erratic Blocks of the Alps and Jura.—The large Alpine boulders found on Mount Jura, forty or fifty miles from their native rock, have been a stumbling block to geologists for the last half century. As the object, though often discussed in books of science, may be new to some individuals, we shall premise a short account of the phenomena.

Mount Jura rises at some points to the height of 5000 feet* above the sea, and 3500 above the great valley of Switzerland on its south side. The Alps run parallel to Jura at the distance of fifty miles, and their higher summits have an elevation varying from 11,000 to 15,000 feet above the sea; but the northern skirts of the chain are a great deal lower, and their distance from Jura scarcely exceeds thirty miles. The following diagram will convey an idea of their relative position :

FIG. 8.



M V B P, The chain of the Alps extending northeast and southwest.

1 1, 2 2, 3, The chain of Jura, running parallel to the Alps.

s s, The great valley of Switzerland separating the two chains.

G, The lake of Geneva; N, the lake of Neuchatel.

The Alps consist of primary rocks, granite, gneiss, &c., in the centre, flanked by secondary. Jura consists of different formations of limestone, all belonging to the oolitic series.

The two chains, in distance, bearing, and position, may be compared to the Ochil and Lammermuir hills. If we suppose the Ochils to be *twice*, and the Lammermuirs *six times* as high as they are, and the valley between them, constituting the basin of the Forth, to be three or four times as deep as it is, we shall have a pretty good idea of the physical features of the district under consideration.

Now the fact which has so long exercised the ingenuity of geologists is this. Hundreds of huge fragments of primary rocks, distinctly recognizable as portions of the Alps, are found perched on the southern declivities, or resting in the interior valleys of Jura, forty or fifty miles from their native locality; and geologists

* The measures are always in French feet, which may be converted into English by adding one fifteenth.

have been perplexed to discover by what agency these erratic blocks have been transported across the great Swiss valley, and placed in the singular situations where we find them. The magnitude, external appearance, and distribution of these masses, present circumstances worthy of notice.

Von Buch, Escher, and Studer, have shown, from an examination of the mineral composition of the boulders, that those on Western Jura, 1 1, have come from the region of Mont Blanc, *m*, and the Valais, *v*; those on the middle parts of Jura, 2 2, from the Bernese Oberland, *b*; and those on Eastern Jura, 3, towards Aargau and Zurich, from the Alps of the Petits Cantons, *p*. The blocks have thus been derived from the parts of the Alps nearest, generally speaking, to the localities where we now find them, as if they had passed across the valley in a direction at right angles to its length.

The blocks are generally angular, and therefore had not been exposed to much attrition, either from agitation amidst gravel, or from mutual action. Many of them are of prodigious magnitude. The famous mass of *Pierre à Bot*, containing 50,000 cubic feet, and weighing probably 4,000 tons, equals a goodly mansion in size, namely, one of 30 feet in front, 40 in depth, and 40 in height. It rests on a part of Jura 2,177 feet above the sea, and about 900 feet above the level of the lake of Neuchatel, *n*. Near Chaumont there is a group of granite rocks, which, from their magnitude, their number, and their juxtaposition, look like a hamlet of cottages. The large Alpine boulders of Jura, in short, may be counted by hundreds, and the small ones by thousands.

The boulders are distributed in zones on the terraces, which, like the steps of a stair, form the out-goings of the different formations. The highest are disposed in rings, as in figure 7, round the lower summits of Jura, at a height between 3,000 and 3,300 feet above the sea. The other zones occur on the terraces below this; the first at elevations from 1,900 to 2,400 feet; the next at 1,600 to 1,800 feet; and the last descends to the level of the lake of Neuchatel, 1,324 feet above the sea. Moreover, these travelled blocks penetrate into the transverse and into the interior valleys of Jura, and some are even found at the back of the chain, near the Doubs.

Saussure attributed the transportation of these boulders to a *débacle*, or great current, rushing from the Alps; and Von Buch, finding that one current would not account for the phenomena, assumed the existence of several. But the inadequacy of such explanations is obvious.

FIG. 9.



A, The Alps; *J*, Mount Jura, with the great valley, fifty miles wide, between them.

E, The southern declivities of Jura, upon which most of the erratic blocks rest.

V, The interior valleys, and B, the back of the chain, where some of them are found.

N, The lake of Neuchâtel.

G, The lake of Geneva.

The difference of altitude between the Alps and Jura, distributed over a space of fifty miles, gives an inclination of no more than two degrees. Now, no current could force, or rather float masses of stone, weighing 1,000 tons, across an uneven valley of such breadth, although the difference of level were much greater. Even if the valley had then been filled up with gravel, or other solid materials, and formed a regular inclined plane, as Ebel and Dolomieu assumed, the blocks could not have been moved over it by water; or if moved they would have been rounded by attrition; and instead of being disposed in zones, they would have been accumulated *pel mel* at the bottom of Jura. It must be kept in mind, that the erratic blocks are found on the Italian side of the Alps as well as the Swiss, and that currents and inclined planes would be required in both directions.

A more recent hypothesis, which assumes that the boulders were transported by icebergs when the great Swiss valley was under the sea, is much more plausible. Agassiz objects to it, that it does not account for the coat of sand and gravel covering the sides of the mountain on which the large blocks generally rest, nor for the striated, grooved, and polished surfaces, nor (he might have added) for the *lapiaz* and *creux*, and the lateral moraines which deviate from a horizontal position.

It will be anticipated that Agassiz transports the boulders across the great valley on a bridge of ice. He observes that the eastern Alps, as they have disturbed the diluvium containing bones of elephants, must have been raised up since that deposit was formed, and their upheaval is the last *cataclysm*, or geological convulsion, which has visited Europe. Previous to this event, an immense mass of ice had covered the surface of the northern parts of the old and new world; but when the upheaval of the Alps took place, this formation of ice was raised up like the other rocks; that the fragments detached from the fissures upheaval (*fentes du soulèvement*) fell upon its surface, and without being rounded—since they were not exposed to friction—moved along the inclined surface of the sheet of ice, in the same manner as the fragments of rock which fall upon glaciers are carried to their sides in consequence of the continual movement produced in the ice by its alternate thawing and congelation, at the different hours of the day, and the different seasons.

“After the upheaval of the Alps, the earth must have recovered a higher temperature; the ice in melting produced large funnels (*entonnoirs*) at the places where it was thinnest; valleys of erosion were excavated at the bottom of these openings, in localities where

no current could exist unless enclosed between walls of ice ; and, when the ice disappeared, the large angular blocks were found resting on a bed of rounded pebbles, of which the smallest, often passing into a fine sand, formed the base."

The description of the supposed phenomena attending the upheaval of the Alps, though it forms the very kernel of his theory, is less clear than the other parts of M. Agassiz's work, which is generally very perspicuous ; and instead, therefore, of giving the substance of his statements in our own language, we have translated the two most important passages literally. In a paper read before the Helvetic Society of Natural History in 1837, containing the germs of the theory, more fully unfolded in his new work, he thus expresses himself :

" The appearance of the Alps, the result of the greatest convulsion which has modified the surface of our globe, found its surface covered with ice, at least from the North Pole to the shores of the Mediterranean and Caspian Seas. This upheaving, by raising, breaking, and cleaving in a thousand ways, the rocks which compose the prodigious mass that now forms the Alps, at the same time necessarily raised the ice which covered them ; and the debris detached from so many deep upbreakings and ruptures, naturally spreading themselves over the inclined surface of the mass of ice which had been supported by them, *slid along the declivity to the spots where they were arrested*, without being worn or rounded, since they experienced no friction against each other, and, even when arrested, came in contact with a surface as smooth ; or after being stopped, they were conveyed to the margin or to the clefts of the immense sheet of ice, by that action and those movements which characterize congealed water when it is subjected to changes of temperature, in the same manner as the blocks of rock which fall upon glaciers, approach their edges in consequence of the continual movements which the ice experiences, in alternately melting and congealing at the different hours of the day and seasons of the year."—*Edinburgh New Philosophical Journal*, No. 48, p. 378.

The words in italics indicate an opinion that some of the boulders might have *slid* from the Alps to Jura on the surface of the ice, while others adhered to it, and only travelled as the angular blocks resting on glaciers now travel. Nothing equivalent to these words occurs in the *Etudes*, and even the distribution of the fragments by the more tardy process is not very clearly explained. We are not sure, for instance, whether he means that the ancient *mer de glace* rose above Jura, and determined the progressive motion of the ice in a direction away from the Alpine chain at right angles, bearing the boulders first detached over Jura into the basin of the Doubs, and that, owing to the gradual fusion and subsidence of the ice, the later boulders were stopped in their motion by that mountain and settled on its southern declivities. He seems, however, we rather think, to mean, that the glaciers of the Aar, the Kander, and the

Rhone, were lateral and auxiliary to that of the great valley; that the dilatation of the ice (and the motion of the boulders) following the course of the troughs in which it lay, was northwest in the lateral valleys, and northeast or southwest in the great valley; and that the blocks resting on Jura are to be considered as stranded on one side of the great glacier, the motion of the eastern portion of it being northeast, while that of the western was southwest. We see some objections to this conclusion. The transference of blocks from B, for instance, (figure 8,) should not have been right across to 2-2, but diagonally to 3, or 1-1, according as the expansive motion of the ice was northeast or southwest. A theory, however, which explains so many facts, is not to be rejected on account of minor difficulties, which future researches may clear up.

When the *mer de glace* was melting, the first openings through it would be formed where it was thinnest. The water engulfed in these would seek out channels where the fissures or vaults under the ice left room for it, and *valleys of erosion* would thus be excavated, sometimes at variance with the natural declivity of the ground and which would afterwards become the channels of rivers. Such valleys do occur, and the explanation is simple and probable. But account should have been taken of the heat developed along the fissure of upheaval which would produce floods of water at the most elevated points; for when the granite ascended from below, though it was in a solid state it must have brought with it the temperature of the region from which it came. The heat thus generated must have been increased by the enormous friction on the pre-existing primary strata, when they were fractured and bent up; and the ice in contact with these strata, which surrounded the highest summits of the Alps, must have been first melted. Here was an obvious source of formidable *debacles*, which must have produced great changes on the surface of the adjacent countries.

As portions of the old alluvium, containing bones of the fossil elephant, have been found turned up on the flanks of the Alps, Agassiz infers that deposits of clay and gravel existed before the icy envelope was formed; that these must have been broken up and remodelled by the streams arising from the fusion of the ice; and, consequently, that part of the existing alluvial cover is derived from the wrecks of one more ancient.

When the ice retired from the great valley or low country, into the lateral valleys of the Rhone, the Rhine, the Aar, and others, the formation of moraines would begin; and the clay, sand, and gravel thus collected at particular localities would be dispersed and remodelled by the bursting of glacier lakes, occasionally formed in the upper parts of valleys by barriers of ice. Hence the origin of a second portion of the existing alluvial cover.

The deposits of clay and gravel spread over the great Swiss valley, must be due to floods arising from both the causes just mentioned. These floods, Agassiz thinks, must have had a depth of not less than

300 feet, for the sand and fine gravel found on the higher parts of Jura have been washed off from the lower to this height. Masses of ice, forming icebergs, would occasionally float in them, and carry boulders from one place to another.

Sheets of ice occupied the lakes of Geneva, Neuchatel, and others at this time, and prevented them from being filled up by the dispersion of the alluvial matter.

The clay containing the bones of fossil elephants on the sides of the Alps, he considers as contemporaneous with the deposits entombing similar remains on the northern shores of Siberia, and he infers that one and the same catastrophe had enveloped these districts and all the northern parts of both continents, in ice. The catastrophe had arrived suddenly; for, as Cuvier remarks, the Siberian fossils show by their numbers that the animals had lived where their remains are found, and by the actual preservation of the flesh and skin in some cases, that they had rested but a short time on the ground before the ice covered them. The retreat of the ice, however, had been slow, as demonstrated by the moraines forming a series in some valleys, with a gradually decreasing range, both in extent and elevation. The present glaciers may be considered as the puny and feeble representatives of that vast crust of ice which formerly enveloped the northern parts of the globe.

The great encrustment of ice necessarily extinguished organic life, so far as its domain extended. The animal tribes which then perished—the mastodon, *Elephas primigenius*, rhinoceros, and others,—have left their remains in the alluvium, and are found closely to resemble the existing races, which were of course introduced after the ice disappeared, and the region acquired the temperature necessary for their support.

Agassiz thinks that a similar great and sudden depression of temperature probably served the same purpose at earlier periods, by clearing the globe of one zoological group, to make room for another.

Mountains, of whose rocks fragments are found transported to a distance, in different directions, are considered as *centres of dispersion*, by Agassiz. Thus the Alps, whose boulders strew the plain of Switzerland, Italy, Austria and France, form one centre of dispersion, embracing Jura, within its range. The Vosges (in Alsace) which exhibit the same phenomena on a smaller scale, are another. The Cevennes, are probably a third; and the Pyrenees a fourth. We have one of vast magnitude in the Scandinavian mountains, whose travelled blocks are found scattered over northern Europe, from the shores of England to Moscow. In this country Agassiz considers the Grampians, the Cumberland mountains, and those of Wales, as centres of dispersion.

There is a question arising out of the theory, which he has not touched upon. If we suppose the region from the 35th parallel to the north pole to be invested with a coat of ice thick enough to

reach the summits of Jura, that is about 5,000 French feet, or one English mile in height, it is evident that the abstraction of such a quantity of water from the ocean would materially affect its depth. The area of the space extending 55 degrees on each side of the pole, is pretty correctly *two-sevenths* of the whole surface of the globe. Supposing two-thirds of this space to be dry land, and the spongy coat of ice equal to two-thirds of its bulk of water, and assuming, what is pretty near the truth, that the sea occupies three-fourths of the surface of the globe, we find that the abstraction of the water necessary to form the said coat of ice would depress the ocean about 800 feet. Admitting further, that one eighth of the fluid yet remains locked up in the existing polar ices, it follows that the dissolution of the portion which has dissappeared would raise the ocean nearly 700 feet. The only very uncertain element here is the depth of the ice ; but even if this should be reduced one-half, we should still have an agent capable of producing a change of 350 feet on the level of the sea. We are besides leaving out of view the southern polar region, which it is now known embraces a great extent of land. If this was also covered with ice, the change would be much greater than we have assumed.

These very original and ingenious speculations of Professor Agassiz must be held for the present to be under trial. They have been deduced from a limited number of facts observed by himself and others, and skilfully generalized ; but they cannot be considered as fully established till they have been brought to the test of observation in distant parts of the world, and under a great variety of circumstances. Supposing the theory to be substantially sound, the magnitude of the consequences it involves will undoubtedly bring objections to light, which may render modifications necessary, both in its principals and its details. In the mean time, it assists us in resolving some difficulties. It contributes, in a greater or less extent, to explain the dispersion of erratic blocks, the *bizarre* situations they occasionally occupy, the banks of clay and gravel found on the sides and at the mouths of valleys, the *striæ*, polishing and grooving, observed on the surface of rocks *in situ*, and of large stones in the till ; and it promises to throw light on what is at present a very obscure subject, the origin of the older and newer alluvium.

Notice of some Experiments on Subterranean Electricity made in Pennance Mine, near Falmouth. By R. W. Fox.

(Contributed by the Author.)

I HAVE already communicated to the Geological Society of London, some results produced by the electric action of two nearly E. and W. metalliferous veins which have been partially explored in Pennance mine. I have since made other experiments in the same mine, in which ore-points, consisting of copper and iron pyrites in

the two veins, were connected by a pair of copper wires, which, in most instances, acted on a galvanometer or other apparatus at the surface, an end of each wire having been brought up through a shaft for the purpose:—about 50 fathoms of wire were employed, although the ore-points in the different veins, were only about 14 to 18 fathoms asunder in a direct line.

A galvanometer of not much sensibility was generally used; the needle, which was $2\frac{1}{2}$ inches long, moved on a pivot, and had a coil of fine wire passed 48 times round it. Another galvanometer, consisting of a suspended astatic needle, and 140 coils of wire was also employed occasionally.

When the former, which call No. 1, was placed in the circuit, the needle was deflected so as to become stationary at 14° to 15° from Zero; and it revolved rapidly round the circle when the circuit was broken and restored a few times, the direction of the electricity being from the south vein to the northern one. The other galvanometer (No. 2), suffered a permanent deflection of about 40° when in the circuit. The interposition of a plate of platinum or zinc at either of the ore-points, or of a *point*, instead of a considerable surface of metal, did not affect the direction or force of the currents; they were moreover, constant in both these respects during more than eight months that the two veins were connected by the wires, and a part of this time the mine was filled with water in consequence of an accident to the machinery. Ore-points in the two veins situated within two or three feet of the others respectively, were at one time, connected by a second pair of copper wires of the same lengths as the first; both sets of parallel wires being kept apart, and insulated from the sides of the levels or galleries, by poles stretched across the latter at short intervals.

When galvanometer No. 2, was placed in the second circuit, No. 1 remaining in the other, the needle of the latter receded at least 2° , standing at 12° , instead of 14° or 15° ; and the former stood at 5° or 6° less than it did when only one circuit was established. On breaking either of the circuits, the deflection of the needle in the other circuit was increased to its original amount. And when *both* pairs of wires were connected with only *one* of the instruments, the effect was almost precisely the same as that produced by one pair alone,—not greater certainly.

A copper and zinc pair of plates of about six inches surface, separated by a piece of cotton cloth moistened with water, was placed in the circuit, and when the currents from this source and the veins *coincided* in direction, the needle of galvanometer No. 1, stood at about 10° , that is, at less than it did when acted upon by the subterranean electricity alone, and when the deflection caused by the latter was afterwards opposed by the action of the plates, the needle went back to Zero, and even sometimes passed a little beyond it in the opposite direction. These anomalies may perhaps be referred to the low conducting power of the moistened cotton, which, small as its thickness

was, very probably interrupted the transmission of the electricity more than the 14 or 18 fathoms of strata or "*Country*."

On taking the voltaic elements from the circuit, and connecting them with the galvanometer, so as to form a separate circuit acting in an opposite direction to the electricity from the mine, the deflection showed a difference in favour of the latter, and indeed, this was the case when the interposed cloth was moistened by a very weak solution of common salt.

The electro-magnetic and decomposing effects of these subterranean currents also afforded unequivocal evidence of their energy. A helix of copper wire fixed round a small horse shoe shaped bar of iron, was placed in the circuit formed by the wires from the veins, when the bar became so magnetised as to cause a compass needle $1\frac{1}{2}$ inch long, at the distance of nearly half an inch, to oscillate through an arc of about 70° , when the circuit was alternately made and broken a few times.

A solution of hydriodide of potash was found to have been decomposed after it had been left in the circuit for rather more than a day.

The endosmose action occurred in various experiments, but it may be sufficient to give one example. Sulphate of copper in solution was put into both branches of a U shaped glass tube with clay in the bent part of it, the surface of the fluid in one branch, standing half an inch above that in the other. A piece of silver wire was plunged into each of them, the upper end passing out through sealing wax, with which the extremities of the tube were stopped, and the apparatus was placed upright in the circuit, with the wire in the higher column of the fluid connected with the negative wire. In the course of a few days this column was found to have risen one eighth of an inch, the other having fallen in an equal degree, showing that the greater pressure of the higher column was superseded by the force of the electric action.

When small cylinders of copper pyrites were substituted for the silver wires in the branches of the bent tube, not only did the endosmose action occur, but the copper ore, forming the negative pole, had its surface gradually changed to vitreous copper in the course of two or three days* the other ore-pole remaining unaltered. The same change was produced, and apparently with equal facility, when solutions of other salts, as carbonate of soda, or common salt, were substituted for that of sulphate of copper, in both branches of the tube. The cylinders of copper pyrites used in these experiments, were long enough for the upper ends to project above the mouths of the tube, where the opposite wires were attached to them respectively, and these were well coated with sealing wax dissolved in alcohol, to prevent the access of moisture to any part of the metal, and indeed, all but the lower portions of the ore were coated in like manner.

In some instances, the cylinders of copper pyrites were allowed to

* Some of the ore thus changed was at the last Polytechnic Exhibition.

remain in solutions of sulphate of copper in the bent tube for several weeks, when deposits of oxide of iron were found coating the inside of the tube about the negative pole. These results remind one of the ochery appearance observed in rocks inclosing much vitreous copper, a fact noticed by my friend Joseph Carne; and it may be worth while to enquire how far the proportion of "*gossan*" in copper veins may be connected with the quantity of vitreous ore contained in them.

Since the foregoing experiments were made, I have obtained an electro-type copper plate $1\frac{1}{2}$ inch long, $1\frac{1}{4}$ wide, and $\frac{1}{30}$ of an inch thick, by the agency of these subterranean currents. The apparatus consisted of a porous earthenware vessel, resting on wooden legs in a larger one; both were partly filled with solutions of sulphate of copper, an engraved copper plate attached to the negative wire being placed in the outer vessel, and another plate of copper attached to the positive wire, in the inner one. After a few days it was observed that crystals of copper had been formed on the negative plate, but it was nearly two months before the apparatus was removed from the circuit, when the deposited metal was detached from the plate, having received its impression, VI INSITA TERRÆ. Whilst this experiment was in progress at the surface, the water, as I have before mentioned, invaded the mine, but without interrupting the process; it appeared, indeed, that the electric action was rather increased than diminished by this circumstance.

Before the influx of the water, an ore-point in the north vein was connected with *rock* near the south vein, (generally the wall of the vein,) and an ore-point in the south vein was likewise connected with *rock* near the north vein, in both which cases, currents more or less feeble, were detected passing towards the latter through the wires, which were insulated, as before, by wooden poles stretched at intervals across the galleries. It is probable that the moisture on the rocks conducted the electricity from the ore to the metal, however imperfectly, and when different metals, as platinum and zinc, were successively substituted for the copper in contact with the rocks, the currents were modified in their force according to the metal employed, but were seldom changed in their direction. The action was most decided when the place of contact with the rock was near ore; and sometimes the end of the wire, or rather the piece of copper attached to it, was rubbed by an assistant against the walls of one of the veins, or the sides of a "*cross-cut*" between them. Under these circumstances the astatic needle was several times suddenly much deflected, and the parts of the rocks from which this increased action proceeded, having been marked, they were broken away, when iron-pyrites was in every instance found imbedded in them; and there can be no doubt that the smallest branch of copper or lead ore might have been detected in like manner.

On several occasions the ends of the opposite wires were placed in contact with the rocks near the two veins, when there still appeared

to be a tendency in the currents to pass in the same direction, but often they could not be detected, or were too feeble for their direction to be determined with certainty. Pieces of copper pyrites attached to the wires, and imbedded in wood, were likewise used instead of the metal, for producing contact with the rocks, and with still less effect; and when the contact was made with platinum and zinc in succession, the currents were in opposite directions, and in accordance with the action of those metals respectively; so that the existence of independent currents under the circumstances described, though more than probable, was not clearly proved. Electricity, generated by a pair of zinc and copper plates, was transmitted through the rocks between the two veins from N. to S., and also from S. to N., in order to detect any independent currents traversing the rocks by a *differential* effect on the needle. This method appeared likely to be a very delicate test of electric action in rocks, but no decided results were obtained, the currents passing in opposite directions apparently with equal facility, at least, the few experiments hitherto made in this way, have not led to any satisfactory conclusions relative to the point in question. It should be remarked, however, that the astatic needle employed, was inconveniently sensitive, and was often set in motion when the cause was not very obvious. With needle No. 1, the case was widely different, as it could scarcely be moved by any subterranean currents that were not tolerably energetic, such as were produced when *both* the wires were in contact with *ore-points*, and then, as has been stated, it often revolved rapidly.

It has been long known that electric currents will traverse a very considerable thickness of rock or strata,* but in what degree this property may be modified by the nature or texture of the rocks, the saline contents of the subterranean water, or the proportion of ores included in the circuit, remains to be ascertained. If the influence of these different circumstances should greatly vary, electric currents generated by given elements might be rendered available on various occasions;—to ascertain, for instance, the connection of saline springs not very distant from each other, often appearing at the surface, or in mines; or of a metalliferous vein discovered in one place, with a vein which has been worked for ore in another. The conducting power of the circuit at Pennance mine, already described, was in this way found to equal that of a tolerably strong solution of common salt, the current in the latter experiment having to traverse an inch of the solution and short copper wires to complete the circuit. The conducting power of the rocks or strata in this case, therefore, appeared to be very great.

* Many instances of this occur in my paper "on the electro-magnetic properties of metalliferous veins," published in 1830, in the Phil. Transactions, p. 399. I have long ago seen a *very feeble* current act on a sensitive galvanometer after it had traversed nearly a quarter of a mile of strata, and stronger currents would probably be detected in like manner after having passed many times that distance under the surface.

When some sulphate of copper was added to the solution, the conducting power of the latter exceeded that of the strata. Glass tubes filled with solution of salts in different known proportions might be used as tests in experiments on the relative conducting power of different strata, and they might be referred to as standards in describing the results.

On the Electricity evolved by the Friction of Water and Steam against other bodies. By MICHAEL FARADAY, Esq., D.C.L., F.R.S.*

THE object of the experiments related in this paper, is to trace the source of the electricity which accompanies the issue of steam of high pressure from the vessels in which it is contained. By means of a suitable apparatus, which the author describes and delineates, he found that electricity is never excited by the passage of pure steam, and is manifested only when water is at the same time present; and hence he concludes that it is altogether the effect of the friction of globules of water against the sides of the opening, or against the substances opposed to its passage, as the water is rapidly moved onwards by the current of steam. Accordingly it was found to be increased in quantity by increasing the pressure and impelling force of the steam. The immediate effect of this friction was, in all cases, to render the steam or water positive, and the solids, of whatever nature they might be, negative. In certain circumstances, however, as when a wire is placed in the current of steam at some distance from the orifice whence it has issued, the solid exhibits the positive electricity already acquired by the steam, and of which it is then merely the recipient and the conductor. In like manner, the results may be greatly modified by the shape, the nature, and the temperature of the passages through which the steam is forced. Heat, by preventing the condensation of the steam into water, likewise prevents the evolution of electricity, which again speedily appears by cooling the passages so as to restore the water which is necessary for the production of that effect. The phenomenon of the evolution of electricity in these circumstances is dependent also on the quality of the fluid in motion, more especially in relation to its conducting power. Water will not excite electricity unless it be pure; the addition to it of any soluble salt or acid, even in minute quantity, is sufficient to destroy this property. The addition of oil of turpentine, on the other hand, occasions the development of electricity of an opposite kind to that which is excited by water; and this the author explains by the particles or minute globules of the water having each received a coating of oil in the form of a thin film, so that the friction takes place only between that external film and the solids, along the surface of which the glo-

* Proceedings of the Royal Society.

bules are carried. A similar, but a more permanent effect is produced by the presence of olive oil, which is not, like oil of turpentine, subject to rapid dissipation.

Similar results were obtained when a stream of compressed air was substituted for steam in these experiments. When moisture was present, the solid exhibited negative, and the stream of air positive electricity; but when the air was perfectly dry, no electricity of any kind was apparent. The author concludes with an account of some experiments in which dry powders of various kinds were placed in the current of air; the results differed according to the nature of the substances employed, and other circumstances.

On the Manipulations of the Dipping Compass. By JOHN LOCKE, M.D., Prof. of Chem. in the Med. Coll. of Ohio, and Lecturer on Nat. Philos. in the Ohio Mechanics' Institution.

MR. LOCKE observes that, "every practical magnetician is aware of the great difficulty in obtaining consistent results, with even the most improved dipping apparatus;" and, having been requested by several distinguished observers, to communicate his views on the subject, has favoured the scientific world with the following remarks.

In the instrument used by me, the dipping needle moves on small pivots supported by straight polished agate edges, on which they roll like a wheel upon a rail. As such a motion is not only rotary but progressive, the centre of the needle is carried out of the centre of the graduated circle, by the same rotation which brings it to the true dip. To restore the pivots to their place, two Vs and two inclined planes* raised by a lever, engage them at their sides and at their ends, adjust them laterally and longitudinally, raise them just clear of the agate edges, and, by a reverse motion, lower and deposit them, and with them the needle in their proper places. Now the most objectionable anomaly has been, that when the same needle has been once adjusted by the Vs, read, and again simply raised by the Vs, and lowered *apparently in the same place*, and read again, there would be a discordance in the readings, often amounting to five minutes of a degree. I felt the full weight of these anomalies in my first attempts to determine the dip, and found to my mortification, that the mean results of the usual eight reversals and sixteen readings, with two different needles, would sometimes differ as much as six minutes of a degree.

I finally reflected that if the pivots could always be made to rest on *exactly the same* points, the readings must always agree, and that the apparent anomalies must arise from slight imperfections of form, and slight and imperceptible, but really essential, changes of position of the points of support to the pivots. With this view I endeavoured by all possible means, so to use the apparatus as to

* These inclined planes slope up opposite the ends of the pivots, and are really two parts of a V widely separated.

bring the bearings at the same points, and especially that the pivots should perform their rotations on the same transverse section ; or in other words, that the same ring or circle of their circumference, should always rest on the agate edges. To accomplish this, the following points received attention.

1. The Vs and inclined planes, which raise and fix the needle to its place, were so adjusted as to allow of no shake or lateral motion of their own.*

2. The distance between each agate edge and the inclined plane, opposed to the end of the pivot operating as an end check, was made as nearly equal as possible.

3. Although the pivots had some "end chase" or longitudinal freedom between the end checks yet in use, each pivot was always slid over against one particular end check by means of a slight pressure with the pencil against the side of the needle ; say, when the face of the needle is westward the pivot is shoved over till it is checked on the west side, and when the face of the needle is eastward it is drawn over until checked on the eastern side.

4. When the needle has been placed in the compass and its reading noted, it should not be immediately taken out and reversed, but be suffered to remain exactly on its bearings, and the compass be *carefully* reversed in azimuth, and with it, the needle, when the second reading should be made, after which the needle itself may be taken out and reversed. The reversals and readings will then have the following order :

1. Face of the compass east, and face of the needle east.
2. Face of the compass west, and face of the needle west.
3. Face of the compass west, and face of the needle east.
4. Face of the compass east, and face of the needle west.

In the above, it will be seen that although the needle in relation to the face of the instrument and to the "points of compass" assumes four positions, yet to attain them it is removed from its place in the Vs but once, and that is between the second and third reading.

I am aware that it may be objected to all of this, that although I may obtain consistent results, still they may not indicate the true dip, for if the pivots of each needle be alike, although not perfectly cylindrical, and alike placed with reference to the axis of the needle, the mean results would agree to the same error. To this objection we observe first, that it is by no means probable that two pivots intended to be cylindrical should both assume another form, and both be exactly alike, and that these forms, even supposing them to be alike, should be alike placed in reference to the needle's axis ; and secondly, we would remark that although we may involve an error by our consistent mode of observation, it would be a constant one, both in quantity and kind, might be ascertained by a great number of comparisons with the results of other instruments, or by other obvious means, and be corrected by a constant equation.

* I have lately examined one of Gambey's instruments, which was decidedly faulty in this particular.

As an example of the order of reversals observed by me, I give below the result of my observations at Baltimore, April 28, 1841.

At the first square north-east of the Washington Monument, called Howard's Woods. 6h. 37m. to 7h. 47m. A. M.

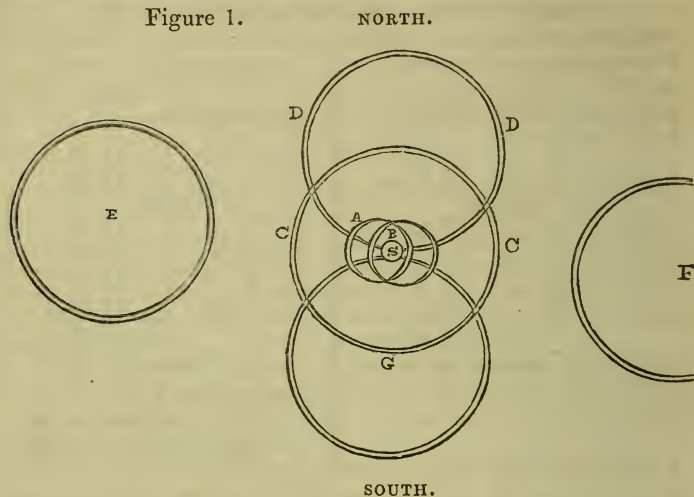
Needle No. 1. A North.			Needle No. 2: A North.		
E.	E.	70°38'.5	E.	E.	71°37'.5
W.	W.	73 03.5	W.	W.	71 16
W.	E.	70 35.5	W.	E.	71 30.5
E.	W.	73 05.0	E.	W.	71 08
B North.			B North.		
E.	E.	72 52	E.	E.	71 55
W.	W.	69 53	W.	W.	71 35
W.	E.	72 36	W.	E.	71 52
E.	W.	69 56	E.	W.	71 34.5
8)572 39.5			8)572 29.5		
Mean=71 34.94			Mean=71 33.7		
			Mean of No. 1=71 34.94		
			2)143 08.64		
			Mean total=71 34.32		

At the Botanical Garden of St. Mary's College, Baltimore, April 28, 1841. 12h. 50m. to 1h. 10m. P. M.

Needle No. 1. A North.			Needle No. 2. A North.		
E.	E.	70°35'.5	E.	E.	71°53'.5
W.	W.	73 00	W.	W.	71 44.5
W.	E.	70 35.5	W.	E.	71 48.5
E.	W.	73 07	E.	W.	71 45.5
B North.			B North.		
E.	E.	72 46	E.	E.	71 37
W.	W.	70 20	W.	W.	71 32.5
W.	E.	72 38.5	W.	E.	71 32.5
E.	W.	70 14	E.	W.	71 16.5
8)573 16.5			8)573 10.5		
Mean=71 39.56			Mean=71 38.81		
			Mean of No. 1=71 39.56		
			2)143 18.37		
			Mean total=71 39.18		

An Account of some Parhelia observed at Milford and Camden, Delaware, March 14, 1841. By A. D. CHALONER, A.M., M.D., Recording Secretary of the Academy of Natural Sciences of Philadelphia, &c., &c.*

Figure 1.



THE following is the account of the parhelia as seen at Camden, seventeen miles northwest of Milford. The day before (13th inst.) was clear, but some two or three days previous there had been some heavy rain. The morning of the 14th inst., the sun rose with unusual brightness with a clear sky; at 8 o'clock A.M., the sky appeared to grow ropy, with uneven crossings in all directions, which first attracted attention. At 8½ o'clock, the sun was surrounded with a circle, which then became of a dim appearance and of a *white* colour; at 9 o'clock two circles, A B, appeared, with a *halo* around the sun, as represented by the letter S, fig. 1. A similar circle, G, was formed in the south, and in about ten minutes was completed, and at the same time the circles C, D, and E, appeared in like manner. At 9½ o'clock the circle F appeared in like manner; the colour of all the circles was of a *sky red*, with the outer edge more of the tint of a rain-bow; in *ten minutes* more, they were of their brightest colours; at 9½ o'clock the circles were all full. The circle F never appeared to be a *full* circle; the altitude of the circles A and B gave an angle of 29°, though somewhat oval, caused by the shadow over them. The circles G, C, and D, cutting the sun's centre, gave each an angle of 69°. The circle E, from its nearest point to the sun, gave an angle of 75°, while F gave an angle of 32°; these circles stood due east and west from the sun, G and D due north and south.

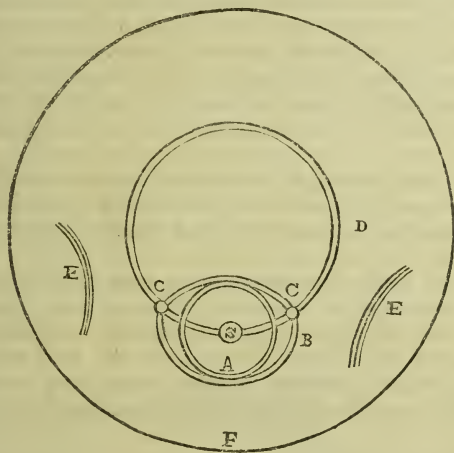
At 10 o'clock the circles disappeared gradually, and at 10½ o'clock

* Communicated by the Author.

they had vanished as they came on ; at 12 o'clock there remained one circle around the sun, which continued until the evening. The next day the sky was hazy, and a storm of rain, hail and snow came on which lasted three days, with a strong north-east wind. The wind on the morning of the 14th inst. was light south-west ; at 10 o'clock it shifted to the north-east, and in the afternoon it was strong north-east.

At Milford, Delaware, the appearance of the sun was somewhat different, as is evident from the following description.

Figure 2.



Imagine a brilliant white circle, D, fig. 2, having the zenith for its centre, and passing through the body of the sun, then a beautiful double iris, A and B, (in figure resembling the telescopic appearance of Saturn and his ring,) encircling the sun, S. Then two parhelia, C C, at the intersections of the circles, in splendour rivalling the great luminary himself ; then parts of two great circles, E E, of a rainbow colour, pointing to the horizon, F, and you have an idea, and only an idea, of the transcendent scenery of the heavens, at once beautiful and sublime, equally calculated to excite the fears of the ignorant, who view it as an omen of disaster, and the admiration of the intelligent, who regard it as the result of the laws of nature. This phenomenon was first observed at 9½ o'clock, A. M., and continued until 10 o'clock A. M. The sky was free from clouds, but the atmosphere was hazy, the weather was mild and calm. It was not seen more than fifty miles distant from Milford ; it was preceded by a shower from the north-west which cleared up with a brilliant sunset. On the 17th inst. an unusually severe snow storm for the season took place.

Another observer at Milford, Delaware, remarks that the sky was

hazy on the 14th of March, at 10 o'clock, A. M., without any appearance of clouds, the sun was of a whitish colour and pale ; so pale or dim that the shadow of an object that intervened could barely be seen. There were two circles around the sun, and in the first circle were *four parhelia*, coloured with all the colours of the rainbow.

The parhelia appeared to be east and west, north and south in the first circle ; at the westerly one there was an arch in the form of a rainbow extending to the north-west, but of a yellowish white colour. For an explanation of this phenomenon, the reader may consult Brewster's Optics, page 227-260.

On examining the monthly meteorological reports for March, 1841, made to the Franklin Institute, from the different counties of Pennsylvania, I find that from the 14th to 17th of March there was an unusual prevalence of *stormy weather*, rain and snow of great depth, but at Silver Lake, Pennsylvania, vice versa, being dry, fine and clear ; a similar state of weather being observed on the 14th inst. at Milford and Camden, Delaware, where the above appearances were observed.

At Little Creek landing, Kent county, Delaware, about three miles east of Dover, and two miles from Delaware Bay, on March 15th,* 1841, the following appearances were observed by Mr Charles P. Hayes.

"There was, in the first place, a very large circle around the sun which was intersected by two others whose tangents were the centre of the sun, each having a central bright spot, resembling the sun shining through a thin cloud. But one of the most striking points in the scene was a beautiful rainbow *immediately under the sun*, which at this time shone bright and warm, the sky being almost clear.

On Wednesday afternoon, the 17th inst., a heavy snow storm took place."

James Nasmyth's Patent Direct Action Steam Forge Hammer.

THE great success which has attended the application of the steam engine in the case of steam ships, and in other instances, has produced a demand for enormous forgings of wrought iron, such as paddle shafts, cranks, &c., that no small difficulty is now felt in the execution of large parts of them, having attained to such a magnitude as to be all but beyond the power and capability of the largest forge hammers to execute them.

The approach of this point of ultimate capability has long been felt, not only by the vast difficulty and expense by the ordinary means, such enormous forgings being so frequently attended by the

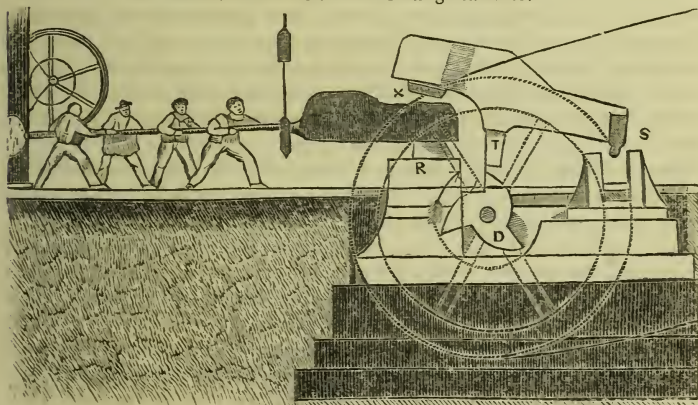
* There is *probably* a mistake in this date (15th inst.) as the *parhelia* was observed at Milford and Camden, Delaware, at the same time, March 14th ; all other accounts coincide as to the time.

destruction of the machinery employed, but also by the frequent occurrence of unsoundness being the certain result of inadequate means, and the exceeding of the limits and capabilities of the machinery hitherto employed for the purpose, arising from a defect inherent in the principle on which such machinery has been constructed, the evils of which have been rendered more and more apparent by every successive attempt to enlarge the apparatus, with a view to endeavour to enable it to cope with the increase in the magnitude of the forgings it was required to execute.

It was with the view to remove *those defects in the principle* on which such forge hammers were constructed, and to produce such a hammer as should, in the most simple manner, attain all that was desirable in our means of forging the very largest class of work, and that in a manner infinitely more convenient, perfect, and economical, that led me to contrive my *direct action steam hammer*, which I shall now proceed to describe, and which has realized my most sanguine expectations of its advantages.

In order to give such of my readers as are not minutely acquainted with the subject, a more clear view of the advantages possessed by this direct action steam hammer over those of forge hammers of the ordinary construction, I must refer them to Fig. 1, which is intended to represent a forge hammer of the largest class, and, generally, ar-

FIG. 1.—View of the old Tilting Hammer.



ranged according to the most improved principle. According to the scale on which this sketch is made out, such a hammer would be fully what is called a seven ton hammer, and, consequently, adapted (so far as its principles of construction will permit) for the execution of the largest class of work.

One chief and universal feature in all such hammers is, that the power which causes them to rise and fall, and so give out blows on the work on the anvil, consists of *rotary motion*, which, originating in the *rectilinear* motion of the piston of the steam engine, is con-

veyed to the hammer by, and through, the medium of *revolving shafts*, wheels, &c., and finally re-converted into its original up and down motion by means of the cam wheel, marked *D* in the sketch ; thus, by a very *roundabout* course, we have brought our power back again into the form it first existed, namely, rectilinear motion, or as nearly so as the radial action of the hammer will permit. And what advantage have we obtained by causing our power to travel to its object by such a roundabout course ? none that I ever could see ; and as to the disadvantages, they are many, and most serious. In the first place, there is great loss of power, on account of the very unfavourable manner in which the momentum of the fly-wheel on the cam shaft *D* communicates its motion to the helve of the hammer, by a jolting action most unfavourable to the economical communication of power ; add to which the vast space of the forge shop, occupied by all the intermediate apparatus of a *complete* steam engine, with its requisite fly-wheels, shafts, beams, and *very costly foundations*, which, in order to endeavour to maintain the apparatus in due order, has to be made of more than ordinary substantiality ; so much so, that, to resist the destructive effect of the vibration given to the entire machinery by the action of the hammer, the foundations have to be made so solid as to cost, in some cases, nearly as much as the whole metallic part of the apparatus.

With respect to the action of such a forge hammer, as seen in Fig. 1, it will be found that one grand defect in principle exists, namely, that, when engaged in hammering a large piece of work, as that seen in the sketch, by reason of the work occupying the greater part of the clear space between the anvil face and that of the hammer, we have thereby a slight blow when we are doing a large piece of work, and a heavy blow when we are hammering a small, or thinner, piece of work, which is just the very reverse of what we could desire. And, in the execution of large work, this is found to be a most serious evil, inasmuch as, from the nature of the case, we would wish to have the most powerful and energetic blows that it is possible to command. The result of this is, that neither is the mass rendered so sound as we could desire, nor is it brought to its required form except by repeated heatings, at the very great sacrifice of time and iron, in so far as, ere the limited blows of the hammer have produced the required change of form, the welding heat has gone off, and all blows after this tend rather to loosen than compact, or solidify, the mass. Again we have another very serious evil, namely, the very confined limits of the space between the hammer face at its highest, and that of the face of the anvil, which renders it quite incapable of admitting, or operating upon, a mass of any great breadth, or height ; and besides having the machinery of the hammer quite in the way, in many cases we have also this other disadvantage, namely, that, except for one thickness of work, the hammer face and anvil are not parallel, as will be evident on referring to the sketch, and considering that the face of the hammer acts radial

to the centre, *s*, Fig 1, in which it rocks. This evil is, to a small extent obviated by means being given to raise up the tail, or centre, *S*; but this process is not only difficult, but can only be done between the heats.

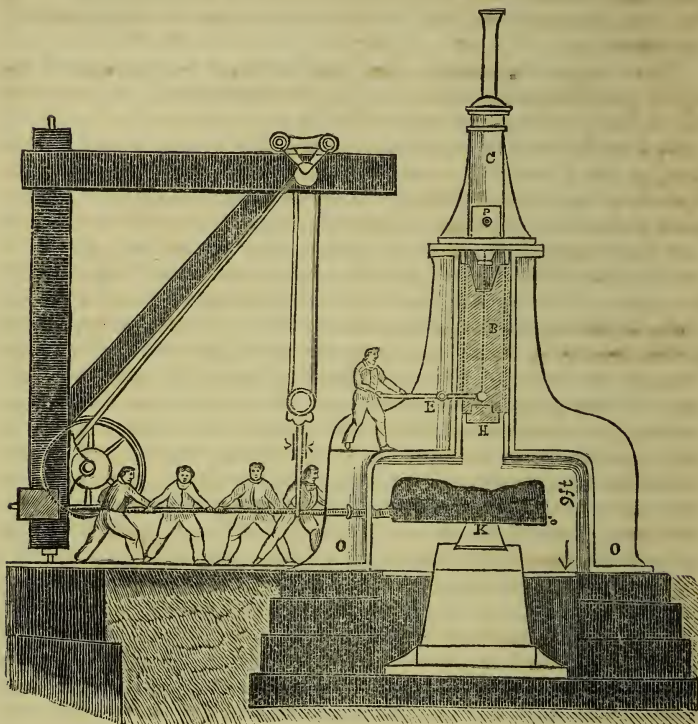
With a view to relieve all these defects, I have contrived my direct action steam hammer, which is represented, in one of its many forms and applications, in Fig. 2.

It consists simply of a cylinder, *c*, turned as it were, upside down; that is, its piston rod comes out at the bottom of the cylinder, instead of (as in most cases) out of the top; this cylinder is supported over the anvil *κ* by two upright standards, *o o*, the end of the piston rod being attached to a block, or mass of cast iron, *B*, guided in its descent by plained guides, or ribs, cast on the edge of each standard. This block of cast iron is the hammer, or blow-giving part of the apparatus, while the cylinder, with its piston and its piston-rod, supplies in the most simple, straightforward, and direct manner, the power by which the striking block, *B*, is lifted or raised up. *Gravity* performs the downward action for us in the most *direct* manner. In order to set this steam hammer in action, steam of such a pressure as operating upon the under side of the piston, will a little* more than balance the weight of the block, *B*, is conveyed from a suitable boiler, (situated in any convenient part of the premises,) through the pipe *r*, into the valve box, in which a slide valve of the most simple form works. The valve, being up, permits the steam to press upon the under side of the piston, and up goes the block, *B*, to *any height* (within the limits of the length of the cylinder) which the forge man may require. The handle, *E*, is now moved in the contrary direction, which not only prevents any further admission of steam, but also permits that which had entered to escape by the pipe *L*; the instant this is done, the block *B* descends with all the energy and force due to its weight and the height through which it falls, and discharges *its full and entire* momentum upon the work then on the anvil, with such tremendous effect as to set the blows of all previous hammers at utter defiance! In fact, the power of such a hammer is only limited by the size we please to make it, as *the principle* is capable of being carried out to any extent; whereas, in the case of such hammers as in Fig. 1, they have their limits, by reason of the very mass of material causing them to be weak *per se*, by the intestinal contraction of the iron which composes their mass, and which, in their action, is so destructive and trying to such a form; the consequence is, they generally break over just behind the neck.

I have only alluded to the means which this steam hammer gives of obtaining tremendous blows. But, energetic and powerful as it is, it is at the same time one of the most striking examples of the manageability of the power of steam; inasmuch as, when we desire to have *any variety* in the intensity of the blow, varying from the

* About five to six per cent. more pressure than will just balance the block, gives all due activity to the upward, or lifting, action of the block.

Fig. 2.—Nasmyth's Direct Action Steam Hammer.



most gentle *nut-cracking tap!* to the most awful smash, we have simply to work the valve-handle in proportion, and, by so regulating the *exit* of the steam, we can let down the block, like closing a well-hung window, or *arrest its downward progress in an instant at any part of its stroke*, and retain it there at any required height, for any required time; on the other hand, by duly regulating the entrance of the steam, we can lift the block to any required height from the face of the anvil, or surface of the work, and so regulate the amount or rapidity of the blows accordingly.

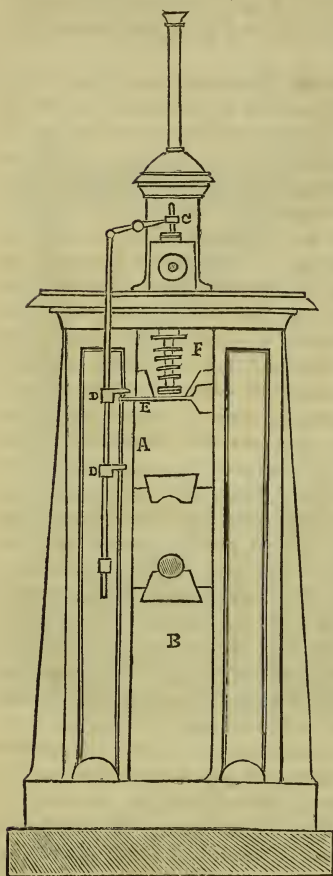
The form and arrangement of the steam hammer, as given in fig. 2, is such as present experience shows to be most convenient; according to the scale on which the sketch is made out, the distance between the standards *oo* gives a clear space of twelve feet, namely, six feet on each side of the centre of the anvil, and six feet height clear over head, as figured in the sketch. But these proportions may of course be varied at will, as the *principle* of this steam hammer affords every facility to extension, or otherwise. The space on each side

of the anvil, in front and behind, being quite clear of all machinery, gives every facility to the introduction and management of the work, when we progress, as will be evident to, and fully appreciated by, practical men.

The comparatively small space which the entire apparatus of the steam hammer occupies, may be judged of by a glance at the sketch, fig. 2, as compared with that of the ordinary construction in fig. 1. Had I turned the standards in the sketch, fig. 2, so as to give a *side*, or edge, view, the contrast in respect to space occupied would have been much more striking. As regards the comparative original cost, any one the least accustomed to such matters will at once see the vast advantage, in that respect, in favour of the steam hammer, to say nothing of its vast superiority as to efficiency and little liability to derangement; in fact, so simple is it, that there is scarcely anything to go wrong. One great source of its durability in this respect is the manner in which the mass of the block is raised, namely, through the medium of *the* most elastic of all bodies—steam; which, in place of any destructive jerk, as in the case of motion conveyed by impulse through solid media, so apparent and destructive in its effect in the case of the apparatus of the ordinary forge hammer, with the steam hammer the lifting motion is performed so smoothly as to be absolutely silent in its action, as if the great block had forgot, for the while, that it had any weight at all. I do not intend here to rival the celebrated Caterfelto, by wondering at my own wonders! but, truly, the action of this simple, but most powerful, machine is not a little striking, both in its action as well as effect. I think experience will prove that I am not too far yielding to sanguine expectations when I state that the vast facilities which this invention gives to the treatment of large masses of wrought iron, will introduce quite a new era in the manufacture and working of wrought iron. We have now, by means of this steam hammer, a power and capability of producing forgings of wrought iron, of *any* dimensions, whose soundness will give the best evidence of the value of the invention in that respect, and from the vast facilities of executing the most ponderous and acquired forms, the saving of time and finish which can be attained under such a hammer, will also prove that a great step has been made in the mechanical arts.

In conclusion, it may perhaps be as well to remark on the valuable and important influence which such a hammer will have upon the quality of iron, as in the case of boiler plates and such like, the quality of which, as regards *soundness*, entirely depends on the efficient manner in which they have been hammered and consolidated in the primary process of faggoting, or shingling, namely, the forming into one perfectly *solid* mass, the block of iron from which such boiler plates, &c., are rolled. Nine-tenths of the defects which are met with in boiler plates, and which have caused such disastrous results, namely, defects from blisters, have arisen from, or may be traced to, imperfect consolidation, resulting from inadequate means

Fig. 3.



of hammering the original mass into a truly solid block, by our not having the *power* to force out all the scoria, which otherwise, lodging between the pile of pieces of which the faggot is composed, gives rise to the most serious defects, which every practical man has had to deplore. It will, in like manner, be scarcely requisite that I state any of the advantages that will arise in our having, by means of the energetic action of the steam hammer, a perfect security against unsound *anchors*, the importance of which requires no words to set forth. In short, we have now at command an almost new power, inasmuch as, by means of this steam hammer, we have an accession to our means of dealing with power in the form and state of *percussion*, such as has never been attained before, and that in the most *simple*, straightforward, and *effective* manner.

Fig. 3 shows the application of the hammer A for forging an iron shaft laid over the anvil, or block, B, and is made self-acting, as will be seen by a reference to the cut, that when the tappets D D come in contact with the pin, or spring, on the block E, the steam valve c is opened or closed.

I may remark, that one boiler can be made to work any number of steam hammers, as the steam has only to be conducted to each by pipes, and the power let on and shut off in the same manner as gas; and in most iron forges, the waste heat of the furnace will more than furnish the requisite steam.—*Extract from the Author's description.*

Improvements in Steam Engines and Steam Boilers. By MR. DAVID NAPIER, Engineer, Mill-Wall. Patent enrolled Feb. 9, 1843.

FIG. 1.

IN this specification three distinct improvements in steam engines are claimed. The first consists of *four* piston-rods which pass through stuffing boxes in the cover of the steam cylinder, and are attached to a cross-head which is connected with the crank by a descending rod. By this means the main shaft is worked by two piston rods on each side. Those called the *second* and *third* improvements deviate from the first in no other particulars than, that in the one there are two piston-rods, and in the other only one.

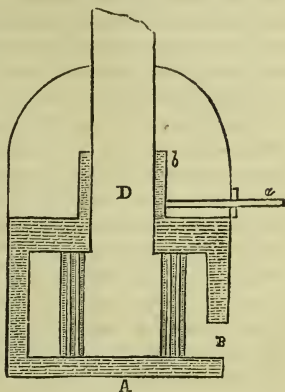
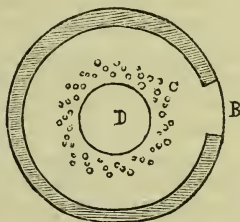


FIG. 2.

That which the patentee claims as improvements in the steam boiler will be understood by consulting figures 1 and 2, in which the letters of reference are the same in both. In fig. 1



A represents the site of the fire. Fig. 2 is a transverse section of the apparatus, which contains a number of vertical tubes, c, through which passes the heated air, according to the following route. The heated air first passes through the flue B; and, after traversing the tubes c, enters the funnel from which it escapes. The funnel D is surrounded by a cylinder, B, called a *heater*; by means of which the water which supplies the boiler through the tube A becomes heated, by the air within the funnel D, and the steam which surrounds it in the boiler. It is obvious, however, that there can be no heat gained but that obtained from the air in the funnel, since any abstraction of heat from the steam would have to be supplied again from the fire.

Improvements in Dying Wool, Woollen Cloths, Cotton, Silks, and other Fabrics and Materials. By PETER KAGENBUSCH.*

This improvement consists in producing a mordant to be used for fixing the colour in cloths, silks, &c., by operating upon the residual mother liquors of alum works; that is to say, the mother

* London Journal and Repertory of Arts, May, 1843.—*Chemist.*

liquor of the alum meal, rough alum, or alum for rocking, from which the Epsom salts have been taken in one or more crops. The patentee uses the residual mother liquor of that manufacture in which muriate of potassa has been employed for the alkaline base, in preference to the mother liquor of that wherein sulphates are used; and he also prefers the liquor of the alum from the natural rock or schist, to the mother liquor of the rough alum, from what is generally termed the "artificial alum process."

The method of operating upon the residual mother liquor is as follows:—A solution of common salt or brine is added to the mother liquor, when boiling, for the purpose of neutralising the sulphuric acid contained in it; the quantity of brine to be added depends entirely upon the amount of sulphuric acid in the liquor; but in general, a saturated solution of salt, equal in quantity to about one twelfth of the mother liquor, will be found sufficient. The liquor is now ready to be used for dying all colours, with the exception of red; and, if not sufficiently strong, it may be brought to the required strength by continued boiling and evaporation.

When the above product or mordant is to be conveyed from one place to another, it is to be boiled until sufficiently concentrated, and is then allowed to crystallise; the crystallised substance should be protected from the action of the atmosphere, and, when required for use, it is to be dissolved in water. In some cases, the solution of salt is added to the mother liquor, previous to taking the last crop of Epsom salts; that is, when boiling up for the last crop: when the salts have been removed, in the usual manner, the residual liquor is ready for use as a mordant.

The patentee claims the improvement in dying, by the use and application of the residual mother liquors of the alum works, and of the product obtained therefrom, as a mordant as above described.

Preparation of Pyrogallic Acid. By DR. JOHN STENHOUSE.

THE usual method of procuring pyrogallic acid is by cautiously distilling either gallic or tannic acids. The pyrogallic acid is obtained partly as a crystalline sublimate, and partly dissolved in the empyreumatic liquor, which passes into the receiver. Pyrogallic acid, thus procured, is seldom free from empyreumatic oil, from which it can only be purified by repeated distillations; in each of which much acid is unavoidably lost. The usual method, therefore, of procuring pyrogallic acid by distillation is troublesome and unproductive. The process which I have found most advantageous for preparing it in quantity is the following:

Finely powdered gallnuts are to be treated with successive portions of cold water till they are exhausted. These extracts are then to be evaporated and strongly dried, till all their hygrometric water is driven off. In this state they form a spongy deliquescent mass, in taste and colour very much resembling catechu. Instead of distilling this dried extract in a retort, it is much better to employ Dr. Mohr's apparatus for subliming benzoic acid. It consists of a cast

iron pan, from three to four inches deep, and from eighteen inches to two feet wide. The dried extract is coarsely powdered, and spread equally over its bottom to the depth of about half an inch. The top of the pan is then covered with a diaphragm of bibulous paper fitting closely over it by being pasted round its rim. The diaphragm may be pierced by a few small pin holes, which greatly facilitates the sublimation. The pan is surmounted with a paper cap, twelve or eighteen inches high, fitted closely to its top, and fastened by means of a cord passed two or three times round it.

The apparatus is to be cautiously heated for ten or twelve hours on a sand or, still better, on a metallic bath. The temperature is to be kept as nearly as possible at about 400° Fahr., though towards the end of the sublimation it may be raised a few degrees higher. The crystals of pyrogallie acid pass up through the bibulous paper, which absorbs the empyreumatic oil by which they are always accompanied. Should the heat have been carefully regulated, the crystals, which are either large scales or needles, are perfectly white should they be slightly coloured, which sometimes happens, they may be easily purified by a second sublimation. This method possesses the great advantage that a pound or more of the extract can be operated on at once. The apparatus is extremely cheap; and as it is not liable to break, it may be used for any number of times. On one trial, 1380 grains of dried extract yielded 69 grains of perfectly pure crystals, and 74 grains, which were slightly coloured, in all 143 grains, or 10.3 per cent. Now as galls yield rather more than half of their weight of soluble matter, the quantity of pyrogallie acid obtainable from them, by this process, is very considerable. I think it right to mention, however, that on a previous trial, when the sublimation was not so carefully conducted, I did not obtain more than half this quantity.

[Phil. Mag.]

Parhelia and Corona seen in England.

East Retford, June 16, 1843.—We have this day had an opportunity of witnessing one of the most interesting atmospherical spectacles it was ever our good fortune to behold,—that of a splendid *corona*, which ultimately ended in two *parhelia*, or mock suns, which is so unusual a phenomenon in these latitudes. Exactly as the sun reached the meridian, the circular cloud, which was about 40° in diameter, appeared to be perfected; at which time its outer edge was encircled with a broad whitish ring, which gradually became more and more burnished, until it had assumed a brightness, in a great measure equal to the sun itself, so that it was almost impossible for the human eye to look at it for any length of time. Remaining in this state for more than forty minutes, this circle gradually developed itself towards the east and west, forming a kind of ellipsis of which not an unfitting representation may be found in the letter O, which gave to the sun an appearance of being surrounded by a

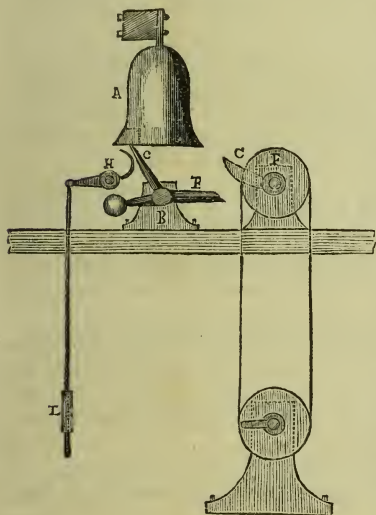
complete rainbow. The broad parts to the east and west were somewhat more deficient in colour, than were the narrow portions to the north and south, which, being confined within narrower limits, exhibited all the most brilliant colours of the iris; the inside being red, and the rest following in regular prismatic order. Shortly after one p.m. five other concentric rings were discovered; the arc of the first being parallel to the horizon, passing directly across the sun's disc, proceeding northwards; and from this two other circles diverged east and west. These rings were of a whitish cast, about 30' broad. Within the arc of one of these last-mentioned circles, were observed two others, crossing each other at right angles: both of these, however, were faint, and gradually disappeared within less than twenty minutes. We were now left with three circles,—the two surrounding the body of the sun, and circumscribing the dense mass of vapour, and the one we have described as extending about 90° northward, and passing directly across the sun's disc. As the sun progressed westward, this last-mentioned circle changed its position from N. to N. E.; and, at a little after two o'clock, the phenomena might, properly speaking, be said to have changed from *corono* to *parhelia*; for, at half-past two, were distinctly seen on the same circle, two mock suns, the one directly north, and the other due east of the sun's centre: that to the north was surrounded by the iris, and well defined; the other less distinct, and of a faint reddish colour. At this period, likewise, the sectional parts of two circles, coloured like the iris, were seen: one in the N. W. subtending an arc of 60°; the other in the S. E. having an arc of 16°. At about three o'clock, the whole scene gradually became fainter and fainter, and finally disappeared in thin haze; the sun breaking forth in the fullness of his majesty, and the heavens once more resuming their wonted serenity. It should here be noticed, that the only modification of cloud to be observed both previous to, and during the progress of the phenomenon, and subsequent thereto, was that of the *cirrostratus*, or wane-cloud, which had a most beautiful and picturesque effect, forming what is usually designated "Noah's Ark," and, being directly in the wind, had the very appearance of the bottom of a ship, beautifully curved to the east and west.

The month of May has been the coldest and the wettest we have had to contend with for many years past; in consequence of which, the surface of the earth having become so completely saturated, the natural exhalations were prevented being carried into effect by the cold which prevailed; and the general low state of the temperature, by night and by day, not assisting such exhalations, but rather tending to prevent them, would cause an immense quantity of water globules to be stationary within the limits of the earth's atmosphere, and waiting the arrival of a higher temperature for its dispersion. This was particularly foreboded by the somewhat extraordinary rise which took place in the mercurial column within twenty-four hours of the *corono* becoming visible. For several days previous thereto, the

barometer had been nearly stationary at about 29·10 inches ; whereas, within a very short period of its approach, it had reached 30·06 inches, at which point it remained for two or three days. The sun, too, having nearly attained to its greatest north declination, and consequently being in the zenith of his power, would tend materially to assist the exhalations then in progress, and draw into a focus the watery globules which formed the dense and circumfluent mass immediately surrounding the body of that luminary, and which, by being refracted and reflected throughout the heavens, was the cause of the grand display we have just been attempting to describe.

[This phenomenon was beautifully displayed at Manchester, and many other parts of England.—EDIT.]

New Mode of Ringing Peals of Bells. By MR. W. JOHNSTON.



THE bell to be rung, and the ringing apparatus, are represented by the accompanying engraving, in which A is the bell, which is firmly bolted to a fixed beam; B represents a low pedestal of wood, which supports a shaft with its lever c, at the head of which is the hammer inside the bell. The shaft carries two other shafts; one of which is furnished with a weight at its end for swinging the hammer with force against the bell; and the other E, when pressed upon by the cam c, raises the hammer again, which repeats its blow as soon as c has slid from the extremity of E. The cam c is fixed to a drum, or cylinder, F, fixed upon a shaft

which reaches the whole length of the row of bells, having a cam for each clapper or hammer. This shaft with its cams revolves by means of a strap which connects it with a drum below to which a winch is attached for turning it. H represents another shaft of much smaller dimensions than the former. It is furnished with a curved spring to soften the blow of the hammer, and keep it detached from the bell as soon as the blow is struck. By means of the rod and handle L, and the lever H, the spring can be made to press against the hammer-shaft more or less, and thus the force of the blow can be modified to any required degree.

On the Fixation of Iron Mordants on Calico. By HEINRICH SCHLUMBERGER.*

THE author of this Memoir has examined the circumstances which are most favourable for the fixation of the iron mordant on calico.

1. The nature of the solution of iron, and the state of oxidation in which it exists in the mordant :

2. The nature of the thickening of the mordant :

3. The state of viscosity of the thickened mordant :

4. The addition of various substances to the ferruginous mordants :

5. The oxidising action of the air on the mordant during the impression ;

6. The hygrometric action of the air during the impression :

7. The drying of the mordants printed on the cloth :

8. The rest of the pieces after being printed :

9. The cleansing or dunging of the printed pieces.

Afterwards follow the accidental causes which may be produced in the operations of dying, and from the brightening of the ferruginous mordants :—

1. The iron mordants most generally employed for dying black, puce, and violet, are the acetate and pyrolignite of iron. These solutions should be in the state of protoxide. When they are oxidised, either by the contact of the air, or by any other cause, the fixation of the oxide on the stuff is but imperfectly effected.

Pyrolignite of iron, on account of the pyrogenous oils which it contains, is more easily preserved in the state of protoxide than the acetate. Hence the preference given to it many cases.

The author has demonstrated that various aceto-ferrugineous solutions might be employed with as much success as the pyrolignite of iron.

After having prepared the mordant for puce, composed of acetate of alumina and iron, the mordant for violet is prepared with the following solutions :—

1. Acetate of iron obtained by the double decomposition of protosulphate of iron and acetate of lead.

2. Acetate of iron prepared by acetic acid and metallic iron.

3. Acetate of iron obtained by iron and the red vinegar of Burgundy.

4. Purified pyrolignite of iron, prepared with pure pyroligneous acid.

5. Purified pyrolignite of iron, boiled for five minutes.

6. Crude pyrolignite of iron, containing a great excess of tar, prepared with metallic iron.

7. Mixture of crude and purified pyrolignite of iron.

These different solutions were diluted with water, and brought to the same areometric degree, and then thickened, one portion with starch and the other with gum of starch, in order to be printed

on calico. After two days' repose, half of the samples were passed into the cowdung bath; the other half remained ten days after printing. After the dying and the brightening, no difference in the result was observed.

Schlumberger concludes from these different experiments, that whatever aceto-ferruginous solution is employed, it will produce a good result if used in the state of protoxide of iron, when applied to the cloth. It is difficult, however, to decide on the employment of any one of these solutions, since everything depends on the kind of preservation followed in the establishments. In the impression of the plate, where the mordants remain for a longer or shorter period in contact with the air in the frame, he prefers pyrolignite of iron.

In printing with the cylinder in which the stuffs are cleansed, a short time after printing, pure acetate of iron should be preferred as oxidising most rapidly.

For printing violet with the cylinder, we have used an aceto-ferruginous mordant, composed of 100 parts of proto sulphate of iron, 9 of deutosulphate of copper decomposed by 124 parts of pyrolignite of lead; the pyrolignite of lead contained a large proportion of pyrogenous oil.

The nature of the thickening exerts a perceptible influence on the results. Starch, in equal circumstances, fixes more of the mordant on the linen than the gum of starch, and the latter more than gum Senegal.

The viscosity of the thickening likewise exerts a very marked influence on the results: a too thick colour imperfectly combines with the tissues, and it is almost impossible to lay down certain rules in this respect. All depends on the kind of design and printing.

Among the substances which may be added to the baths of iron in the preparation of violets, Schlumberger gives the preference to the salts of copper, and in some instances to sugar syrup.

We have used with advantage a mordant composed of pyrolignite of iron, previously boiled with iron filings and powdered charcoal, then filtered over iron turnings; this pyrolignite of iron abandons much pyrogenous oil. Suitably diluted with water, and mixed with a small quantity of acetate of alumina and acetate of copper thickened with gum serving for very delicate lilacs, it took a very bright rose tone in the brightening.

After having examined the hygrometric action of air in the workshops during the printing of the iron mordants, it results that the air should always be maintained at a certain degree of humidity; too great dryness injures the combination of the mordant with the stuff. It is necessary, however, that the solution should not be too long, for the acetic acid being retained between the superficial layer of gum and the stuff, cannot be again disengaged, and the combination can no longer be effected. After having examined the effects produced by a too low or a too high oxidation, the author of

this Memoir concludes that the iron should be in a degree of oxidation intermediate between the protoxide and the peroxide, after the operation of dunging. This supposition is so much the more probable as the same phenomenon is remarked in dying wool black. Experiments made with the view of attaining the degree of heat necessary for the cleansing of iron mordants in the cowdung baths, gave a temperature of 60° C. It is a well proved fact, that acidity should be avoided in the dung bath, as the acetic acid would act as a solvent of the mordant combined with the cloth. The addition of chalk not only gets rid of this inconvenience, but also aids in the fixation of the iron mordant on the cloth.

The author has demonstrated that weak iron mordants do not exert a destructive action on cloths when they are at a low degree. At 15° Beaumé, the alteration of the cloth takes place with aceto-ferruginous mordants: this alteration is less perceptible with the pyrolignite of iron than with the pure acetates of those obtained by double decomposition; that the stuffs printed with mordants become weaker as the oxidation is more rapid; that under the influence of the solar rays, the alteration is more rapid than by exposure to heat in a dark place; that thickened mordants weaken less than those which are not thickened; finally, that a cleansing in a boiling dung bath also causes alteration of the tissues.

Without deciding, however, the alteration of the tissues is attributed rather to the effects which accompany or follow the combination of the protoxide of iron with oxygen to produce the peroxide.

*Aconite in Rheumatism.** By DR. BUSSE.

DR. BUSSE, of Berlin, has published a monograph on the use of aconite, in which he recommends it in acute as well as chronic rheumatism. If there be a foul condition or inflammation of the primæ viæ, that must be removed by appropriate treatment previous to its administration. The formula employed by Dr. Busse, for the last twenty-five years, is a prescription of Richter, and is as follows:—4 scruples of extract of aconite, dissolved in 30 of tartar emetic wine. Of this, at first he gave only small doses, in accordance with Richter's advice, from 15 to 25 drops every two hours, but he soon found he could give double the quantity, not only without danger, but with greater prospect of success.

Dr. Busse now begins with from 30 to 40 drops at a dose, and rapidly raises it to 60. He has found it of equal service in all cases of pain arising from a rheumatic origin, in tooth-ache, cramps of the stomach, &c.

* Hufeland's Journal, and Lond. and Edin. Monthly Journal of Medical Science, May, 1843.—*Chemist*.

Practical Observations on the Construction and Management of Insulated Exploring Wires designed for Researches in Atmospheric Electricity; Illustrated by a minute Account of an Apparatus of this description, which is extended about three hundred and sixty-five yards over the town of Sandwich, in Kent. By W. H. WEEKES, Esq., &c., &c.

NOTWITHSTANDING the design of the present memoir is decidedly of a practical character in regard to fixed atmospheric electrical machinery, I apprehend that a few historical notices relative to such principal forms of apparatus as have been from time to time employed by eminent cultivators of electrical science, for the purpose of collecting the potent, and probably, universal fluid, from the surrounding medium, will neither be considered out of place nor devoid of interest on the occasion.

The electric kite of the celebrated Franklin not only proved instrumental in demonstrating one of the most sublime and important facts in natural philosophy, but became, moreover, the parent of all subsequent contrivances for drawing electricity from the clouds; and, were this simple apparatus at all times available, a resort to other expedients would be little better than superfluous, as with none can we hope to exceed, if, indeed, to rival its excellence for such purposes. The numerous experiments of M. De Romas, Signior Beccaria, Cavallo, and others, among the early electricians immediately after Franklin, as their various interesting records will amply testify, united to confirm the expectations which had arisen from the grand practical idea of that original philosopher. But in connexion with this important topic, I feel that I should be wanting in ordinary justice, were I to omit a notice of the more recent and elaborate researches of Mr. Sturgeon, whose perseverance as a kite experimenter during a series of many years, has been unremitting and unrivalled; while the results of his numerous inquiries constitute a monument of valuable truths in illustration of electrical science generally.

It is a fact well known to electricians, that the French philosophers were the first to distinguish themselves in verifying the hypothesis of Dr. Franklin; and this important event appears to have obtained during the transit of a single thunder cloud, on the 10th May, 1752, about a month prior to the date of the Doctor's completion of his own capital discovery. Messrs. Dalibard and Delor, (both zealous partizans of the Franklinian theory,) at or near the same time, had respectively constructed an apparatus for the express purpose of identifying the matter of lightning with that of frictional electricity, but the machine of the former, as Dr. Priestley observes—and to him I am indebted for much of my historical information—"happened to be the first that was favoured with a visit from this ethereal fire." Vide *Priestley's History and Present State of Electricity*. London, 1769.

The apparatus of M. Dalibard was situated at Marly La Ville, about five or six leagues from Paris. This machine, which the philosopher entrusted in his absence to one Coiffier, an intrepid as well as intelligent joiner, consisted of a perpendicular iron rod, about forty feet in length, terminating below within a kind of centry box, contrived to keep off the rain, while, on the outside, it was secured to some adjacent wooden posts by means of long silken strings, said to be defended from the rain also, but by what means we are not informed. Every allowance ought, doubtless, to be made for a first attempt in the construction of apparatuses designed for researches of this magnitude, and over which still hung the mantle of comparative speculation; but there are few modern electricians who would not pronounce such a machine as M. Dalibard's to be of a highly dangerous character during the operation of great thunder storms, wanting, as it did, within the centry box, the means of effecting a *striking* communication with the earth. In fact, M. Dalibard, in a detail of his subsequent experiments, says, that M. Monnier, a physician, of St. Germain en Laye, Member of the Academy of Sciences at Paris, and Mr. Bertier, of the Oratory at Montmorency, a correspondent of the Academy, were both struck down by strokes of lightning, as they were taking sparks from their apparatus.

Eight days after the successful issue of M. Dalibard's experiment, the apparatus of M. Debor, erected at his own house, situated on some of the highest ground in Paris, also gave similar demonstrations during the passage of a cloud unattended by either thunder or lightning. The iron bar, in the latter instance, had an elevation of ninety-nine feet, and was, consequently, found to answer better than that of M. Dalibard. These philosophers, finding the quantity of electricity obtained in their first experiments insufficient for the object of their researches, subsequently added to the original apparatus a series of insulated metallic rods, communicating with the primary bar. This combination of rods they denominated a "Magazine of Electricity;" and it appears that a similar contrivance was, about the same period, employed by the Abbe Mazeas, for the purpose of conducting the electric fluid from the atmosphere into an upper apartment of his dwelling-house. However, as Dr. Priestley also in substance remarks, while the insulating supports were thus exposed to rain and other casual influences of the atmosphere, the experimentalists were subject to a repetition of vexatious defeats.

It would seem that almost immediately after Franklin had realized, by means of his celebrated kite experiment, his first grand conception of the identity of lighting and the electric fluid artificially produced, he set about the construction of his atmospherical electric bell apparatus, designed for the double purpose of prosecuting his researches and warning him of approaching clouds more than ordinarily electrified. The idea was worthy the mind of the American Philosopher; one of those happy expedients by means of which the economy of time and the ardour essential to scientific investiga-

tion are mutually harmonized and promoted. Dr. Franklin's apparatus, in accordance with his habits and contrivances generally, was remarkable for its simplicity and practical effect. The machine consisted of an elevated pointed rod, which was insulated by passing through a glass tube in the roof of the philosopher's house. On the floor were placed two small bells, one of which was supported by a glass pillar, and the other by a metal one. From the lightning rod a wire descended to the insulated bell, and a small metallic ball, suspended by a silken thread, placed between the two, and caused them to ring when the apparatus became electrized by a "*wave*," which, as Mr. Sturgeon has often truly and very aptly expressed, a passing cloud produces and transmits through the vicinal atmosphere.

From the writings of the Abbe Nollett, we learn that in the same year (1752) iron bars, constructed, as I am induced to believe, upon the same principle as those employed by Messrs. Dalibard and Delor, were used in Florence and Bologna; for the Abbe states that one of his correspondents informed him, that he was struck backwards four or five paces, and that he had never been so much shocked by the experiment of Leyden, as while engaged in fastening a small chain with an oscillating ball to a greater chain which communicated with a bar at the top of the building. It would seem this happened during a thunder storm, for there came a flash of lightning which the operator did not perceive, and "affected the chain with a noise like wildfire."

Dr. Priestley concludes that "it was not owing to any want of attention to this subject that the English philosophers were not the first to verify the theory of Dr. Franklin. They happened to have few opportunities of trying experiments. In the few they had, they were disappointed by the rain wetting their apparatus, which was not better constructed than that of the French."

At length success crowned the assiduity and scientific accumen of Mr. Canton, who, as early as the month of July, 1752, had mounted an apparatus on his house in London, for the purpose of instituting researches in atmospheric electricity. To this gentleman is originally due the happy idea of having adopted a tin cover, or funnel, for the purpose of protecting from rain and other meteorological contingencies, the glass tube to which was attached his rod, or "conducting wire;" and, notwithstanding time and experience have enabled us greatly to improve and vary the detail of his contrivance, the advantages of the fundamental principles have not been superseded even in the best apparatus of modern invention.

Signior Beccaria of Turin, whose name will ever remain illustrious in the early annals of electrical science, made use both of kites and pointed rods, employing, as we are told by himself, a great variety of these at the same time, and in different places. One of his permanent rods terminated in his observatory; and, although I have not been able to meet with any particular description of the apparatus, I am satisfied that his arrangements must not only have

been extensive, but well designed, for the machine “never failed to be electrified upon every approach of a thunder cloud, or any of its branches; and the stream of fire from it was generally perpetual, while it was directly over the apparatus.”

One of Signior Beccaria’s arrangements consisted of a hempen cord 1500 Paris feet long, stretched over the river Po; and this being insulated, he found as strongly electrified during a shower, without thunder, as a metallic rod, to bring lightning into his house, had been in any thunder storm. I may here remark, that several of the most magnificent electrical currents drawn from the atmosphere by my own apparatus, have appeared incident to heavy autumnal rain squalls unaccompanied by lightning discharges.

But among the most elaborate series of experiments which were undertaken in the infancy of this science, with the view to determine the electrical condition of the atmosphere, independent of thunder storms, are those of the Abbe Mazeas, during the months of June, July, and October, 1753. The experiments of this philosopher were made at the Chateau de Maintenon, where his apparatus, consisting of an iron rod 370 feet in length, and raised ninety feet above the horizon, was suspended by means of silken cords, sheltered from the rain between a very high room in the castle and the adjacent steeple of the town. From the principal rod, and through the medium of a secondary wire, depended a large key, and from this somewhat uncertain and irregular instrument, in lieu of a metallic ball which would have constituted a far better terminus to the apparatus, the Abbe, notwithstanding, drew many valuable conclusions. He had, moreover, at one time a “magazine,” similar to that designed by Messrs. Dalibard and Dalor, constructed in an upper room of his house, into which he brought the lightning by means of a wooden pole which projected from the window, and carried at its extremity an insular and pointed iron rod, twelve feet in length. Had the experiments of the Abbe Mazeas been longer continued, he would in all probability have corrected several of his early impressions, as when for instance, he states that, “the direction of the winds, whether east, west, north or south, made no sensible alteration in the electricity of the air, except when they were moist.” Certain it is that the atmospheric apparatus which I have now had in constant operation for several years, has never failed to yield me a free current of electricity, and not unfrequently during a whole day, a continuous and uniform discharge of moderate, but very intense sparks, under the influence of a blue sky and a smart breeze from the *eastward*; while, the wind blowing from another point of the compass, precisely similar conditions of the air have failed to produce more than an occasional and ordinary divergence in the electroscopes. Habitual attention to phenomena of this kind has, indeed, shown me the necessity of invariably removing my electroscopes from the wire terminus whenever the meteorological conditions before stated seem likely to obtain; otherwise the destruction of my gold leaves is sure

to follow. Under such circumstances I have seen them forcibly driven off, deflagrated, and sometimes partially fused, into the surrounding glass screen.

Happily no other form of machine which ingenuity has yet devised for the purpose of promoting our researches in atmospherical electricity, possesses such a melancholy and unenviable distinction as that constructed by the unfortunate Professor Richman, of St. Petersburg, who became a martyr to the imperfections of his apparatus—at least, such is my own conviction of the matter—while engaged in observing the electrical phenomena incident to an approaching thunder storm, on the 6th of August, 1753. The Philosophical Transactions of the period contain two accounts of this fatal event, one communicated by Dr. Watson, and the other translated from the Dutch language, and in all material points their testimony is concurrent. Dr. Priestley, in his History of Electricity, deduces from these papers the following concise description of Richman's apparatus. "The professor had provided himself with an instrument which he called an electrical gnomon, the use of which was to measure the strength of electricity. It consisted of a rod of metal, terminating in a small glass vessel, into which (for what reason I do not know) he put some brass filings. At the top of this rod, a thread was fastened, which hung down by the side of the rod when it was not electrified; but when it was, it avoided the rod, and stood at a distance from it, making an angle at the place where it was fastened. To measure this angle, he had the arch of a quadrant fastened to the bottom of the iron rod." It should be here stated, that an atmospheric wire, the nature of which is not exactly known, entered the room, and conducted the electricity to the metal rod and gnomon supported by the glass vessel. Nothing, I should imagine, would be more certain in the estimation of a modern electrician, than the dangerous character of such an apparatus, during the transit of a thunder cloud, being, for anything that appears to the contrary, like Dalibard's, Delor's, and others of that period, utterly deficient in the means of effecting a *striking* communication with the earth; while, in augmentation of the mischievous tendency of this lightning rod, it was unfortunately *insulated* from all vicinal conducting bodies, by means of a glass vessel containing some brass filings. What could have been the intentions of the professor in regard to this latter arrangement it seems now impossible to determine. The accounts above referred to state, that "half of the glass vessel in which the rod of the gnomon stood, was broken off, and the filings of the metal that were in it were thrown about the room;" an unequivocal demonstration of the mechanical effects of the lightning flash in its unavoidable passage to the earth, for which, unhappily, no artificial provision had been made.

It would seem, from various scattered and undefined notices, that other philosophical individuals, both in England and on the continent of Europe, have, from time to time subsequent to the Frank-

linian discovery, been induced to erect "lightning rods," as they were familiarly denominated, with the laudable view of increasing our knowledge in atmospheric electricity. But there is no evident reason for supposing that more extensive arrangements than those hitherto described, were ever undertaken, or even contemplated, until the philosopher of Broomfield, at an early period of the present century, planned and executed his splendid atmospheric battery, consisting of a vast line of exploring wires, stretched abroad, horizontally, amongst the majestic trees which ornament his picturesque grounds, and finally terminating in his magnificent electrical laboratory.

Perhaps none but those who possess the happiness of a personal acquaintance with the modesty and purely philosophic spirit which actuates Mr. Crosse in his multitudinous scientific researches, would fail to express surprise that his extraordinary machinery has, until recently, attracted such comparatively small notice from electricians generally, and more especially those among his own countrymen. The only typographical account of the apparatus and experiments of Mr. Crosse, relative to atmospheric electricity, I believe is that published by his late friend G. J. Singer, in his "*Elements of Electricity and Electro-Chemistry*,"—London, 1814,—a highly interesting and valuable book of science, which has now become exceedingly scarce. The early death of poor Singer can alone account for a single edition of his excellent work having appeared, notwithstanding the lapse of some thirty years, and the rapidly increasing attention to electrical pursuits. Not only were many of Singer's able conclusions derived from researches carried on in conjunction with Mr. Crosse, but several of his best experiments were first performed by means of the gigantic machine and capital series of apparatus accumulated in the Broomfield laboratory, some of which had been constructed and mounted by his own hands. I cannot refrain from remarking incidentally, that a feeling of soft and philosophic melancholy rarely fails to steal over Mr. Crosse's auditors, while he dwells, in the glowing language of enthusiastic regret, on his reminiscences of George John Singer, and the premature decease of that talented electrician. I shall here transcribe the general description which Singer has given of the now celebrated Somersetshire apparatus. "My friend, Andrew Crosse, Esq., of Bromfield, near Taunton, a most active and intelligent electrician, has lately made very numerous observations with a remarkably extensive atmospherical conductor, consisting of copper wire one-sixteenth of an inch thick, stretched and insulated between stout upright masts, of from 100 to 110 feet in height. The most unwearied exertion has been employed to give unexampled extent and perfection to this apparatus; the insulated wire has been extended to the extraordinary length of one mile and a quarter; and a variety of ingenious contrivances have been applied to preserve the insulation; but the length of the wire rendered it so liable to injury, and subject to

depredation, that it has been found expedient to shorten it to 1800 feet; and, until the present time, no means have been found that will sufficiently preserve the insulation during a dense fog or driving snow."

"There are some minor inconveniences attendant on the use of this apparatus, which are obviated by fixing it very securely, and providing a contrivance by which it can readily be raised or lowered to cleanse the insulators; for these are sometimes rendered conducting by spider's webs; and the secure fixing of the wire is essential to resist the weight of innumerable swallows that occasionally perch upon it, and of wood pigeons and owls, which frequently fly with considerable force against it." In a subsequent paragraph Singer, in allusion to the Broomfield apparatus as it existed incident to his own time, says, "A wire of this kind has been kept strained for eighteen months without injury." However, I can, from the assurance of Mr. Crosse, as well as from a local knowledge and my own experience, state, that such an occurrence is to be regarded as extremely rare; and, notwithstanding the vast *intensity* and consequent chemical effect to be derived from such immense lines of conducting surface, this eminent electrician has latterly confined his operations to some two or three hundred yards of insulated wire, and this not unfrequently suspended, especially in the summer months, directly beneath and passing to and fro amidst the majestic and beautiful tree scenery by which his mansion and laboratory are surrounded. I remember a very interesting conversation on this subject during one of my visits to Broomfield, when Mr. Crosse stated, that he thought the greatest quantity of electric fluid he had ever drawn from the atmosphere at one time, was conducted into his laboratory by means of some seven or eight hundred feet of wire; and, I think, he added, that it occurred at the time of some tremendous rain squalls from the south-west. I may here appropriately remark, that similar meteorological phenomena have more than once been productive of the most splendid electrical and electro-chemical results, in connection with the atmospheric apparatus I have for several years had extended over a considerable portion of the town of Sandwich, an accurate and familiar description of which I will presently endeavour to convey.

Whatever may be the character of the local scenery, no doubt can obtain that free electricity may be drawn and collected from the surrounding atmosphere, in a greater or less degree, obedient to its meteorological conditions, by means of a series of insulated wires properly distributed and connected with suitable arrangements for such purpose. It is, however, tolerably evident, both from reasoning and experience, that certain districts would be vastly superior to others in promoting the success of such operations. Low marshy or other damp situations, presenting considerable surfaces for evaporation, are obviously unfavourable to the objects of such an apparatus, from the circumstance that its insulation would be fre-

quently interrupted thereby, and the wires thus thrown into a neutral condition; although it might be shewn that an additional quantity of electric matter is liberated into the atmosphere when water passes into a state of vapour. This phenomenon has often been beautifully illustrated in fine dry summer weather, accompanied by clear skies, when the insulation of my apparatus is seldom entirely suspended through dampness of the night air. Almost as soon as the first rays of the rising sun fall upon objects bespangled with the morning dew, and light vapours are seen to roll upwards, a corresponding divergence of the electroscopes takes place within doors, gradually increasing with the amount of evaporation.

Again, it may be stated with certainty that vicinal high grounds, or ranges of hills, presenting either pointed summits or sides clothed with foliage and verdure, operate powerfully (especially through the spines and leaves of their tree-scenery, such as groups of tall firs, &c.,) in drawing down considerable quantities of fluid which might be otherwise gained artificially. But after all that might be urged from the influence of local impediments, an adequate supply would frequently result to the careful and intelligent electrician; a striking illustration of which is manifested by the arrangements of my friend Andrew Crosse, Esq.; notwithstanding, I presume that his success in this respect would be even greater in a more open tract of country; the grand object to be sought being, as I conceive, not merely an elevated point of situation, but a sufficient altitude of the wires, or at least one extremity of them, above the earth's immediate subjacent surface.

The town of Sandwich, in Kent, one of the ancient Cinque Ports, and against the olden walls of which the now somewhat remote ocean once heaved its majestic billows, is situate on the south-eastern coast of England, about two miles in a direct line from the shore of that part of the British Channel denominated the *Downs*. The site of the present partially irregular streets and buildings constitutes a tolerably uniform plane, gradually sloping towards the north and north-west, until it terminates on the bank of the river Stour, which has its course parallel to the remains of one portion of the antique walls, and separates the locality from the Isle of Thanet in that direction. As we approach the sea on the east and south-east, a considerable tract of downland interposes, the surface of which, at first moderately undulating, at length forms occasional sharp ridges and groups of sand-hills, which latter crowd upon shore, and by their broken and uncertain outline operate materially to intercept and circumscribe the view towards the horizon on this side of Sandwich. Over these hillocks and sand-downs, at certain seasons, the bleak north-east and easterly winds, after traversing the German ocean and Channel sea, sweep with singular rigidity, and are often found exceedingly favourable to a highly electric state of the atmosphere above the town and neighbouring locality. The only eminence entitled to notice, in connexion with our present object, is

the hill of Richborough, situate some ten furlongs north by west from the town; and from the broad plane upon the top of this hill, the ruins of the celebrated castle which has conferred the name, still exhibit the vestiges of their ancient impregnability. Westward from the group of sand-hills formerly mentioned, an immense tract of rich marsh land, subject to a most effective system of sewage, extends between Richborough and Thanet, until it reaches the site of the famous sea-mark, designated the "Reculvers;" but prior to its arrival at this spot, a broad belt of similar character is given off in a southerly direction, and gradually merges into the vale of the Stour, in a line towards the city of Canterbury. This tract of fertile marsh, doubtless, once constituted the vast estuary dividing Thanet from Kent, and the bed of the *Portus Rutupinus* of Anglo Roman history.

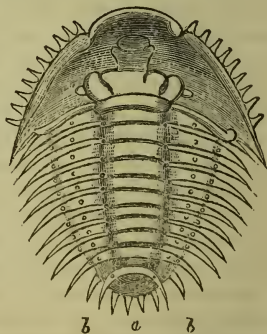
Although the immediate neighbourhood of Sandwich abounds in rural scenery, consisting of timber trees, stump willows, tall poplars, and plantations of firs, larches, &c., very little foliage of any kind occurs within the walls of the town to interfere with the operations of the electrician; while the character of the few groups that actually obtain adjacent to the range of the wires, reduces their influence to a trivial degree of consideration. This now somewhat sequestered place, once a position of no small importance, and long since noted by historians for some curious remains of antiquity, contains three spacious churches, two only of which possess the appendage of a tower, or other claims of architectural distinction; the third being remarkable merely for its low, broad roof of extraordinary span, and an interior of corresponding dimensions. The tower of St. Peter, which is closely surrounded by numerous buildings intersected by several lines of principal thoroughfare, has its site not far removed from the nearly central market-place, and commands from its summit an extensive view over the adjacent country, as well as Thanet, the Downs, and, occasionally, under favourable atmospheric conditions, the opposite coast of France. The church of St. Clement is situate on the highest ground within the walls, and only a few yards distant from the remains of a deep and very ancient fosse, on the south-eastern side of the town. The tower of this edifice presents various features of great masonic beauty, and is supposed to be one of the oldest Saxon buildings now remaining in England; it is, consequently, an object of great interest to the antiquary, notwithstanding the changes which Gothic ignorance and the lapse of ages have entailed upon its original design. Among these, not one of the least to be regretted by an electrician, is the erection of a diminutive and comparatively modern substitute for a spire, instead of a structure more elevated and worthy of its general and picturesque character. I will now state, that against the respective balls from which arise the vane spindles of the two churches above mentioned, are insulated the extremities of the aerial exploring wires connected with the apparatus now to be described. These

wires, which are of annealed copper, and rather more than the sixteenth of an inch in diameter, extend about three hundred and sixty-five yards horizontally over the houses and thoroughfares of the town. Their direction through the air, however, is not exactly that of a right line which would pass from N. W. to S. E. nearly, but forming an obtuse angle closely approaching to 165 degrees, the apex of which obtains at an intermediate station bordering on midway between the church towers; this third insulated position being supplied by a contiguous elevated chimney, a few yards from which is given off a descending branch wire, having communication with suitable operative arrangements, presently to be noticed, both without and withinside the window of an adjacent laboratory. As regards the elevation of the exploring wires above the surface of the earth, I may remark that, if an imaginary right line were to be drawn horizontally so as to connect the bases of the two church towers, the respective terminations of the wires would have an altitude of 136 feet above the supposed line. This is actually the fact with the extremity of the wire supported by the tower of St. Peter, but, owing to an ascent of the ground as we approach the Saxon tower on the opposite side of the town, the wire at that station cannot command a height of more than some ninety feet immediately above the surface; while the entire line may be estimated to possess an *average altitude* of about fifty-five feet above the level of the sea. A tolerably accurate and at once comprehensive idea of the external arrangements hitherto noticed generally, will be gained by turning to the perspective sketch, plate 1.*

(To be continued,—with Plates.)

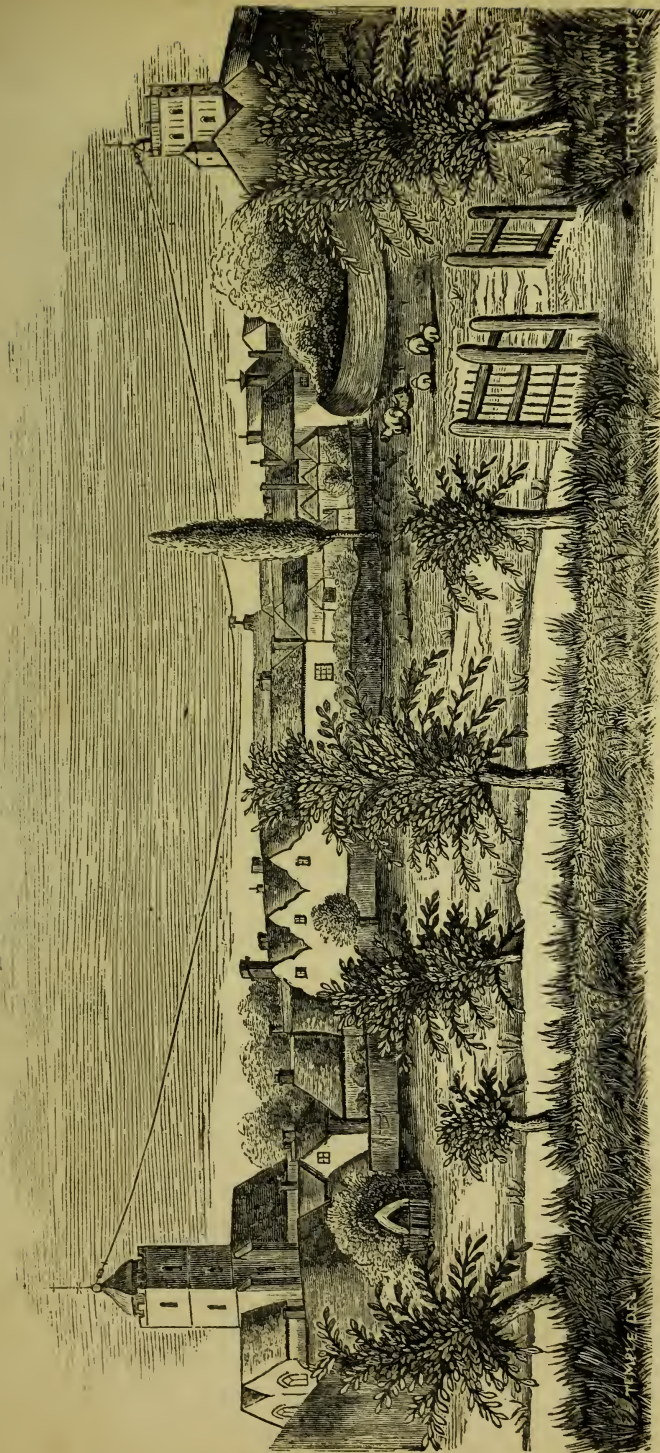
Notice of a new Trilobite, *Ceraurus crosotus*. By JOHN LOCKE, M.D., Prof. of Chem. and Phar. in the Med. Coll. of Ohio.

MESSRS. SILLIMAN,—I enclose to you a drawing of a new species of trilobite, evidently of the genus *Ceraurus* of Green. It is one of the smallest, and at the same time one of the most elegant of this family of extinct crustaceans; this drawing being magnified three times in linear dimensions. Fragments of this species have been repeatedly found in the rocks of this vicinity, especially the fringed margin of the shield; but it was not until last summer that I procured a specimen so



nearly entire as to determine its generic relations. When Dr. Green established the genus of *Ceraurus*, it consisted of only one species, the *Pleurexanthemus*. But now that other species very closely allied to that are found, the justness of his discrimination is very

* This plate, and also plate 2, will appear with the next number.



View of the town of SANDWICH, with MR. WEEKES'S ATMOSPHERIC ELECTRICAL APPARATUS.—See No. 2, page 106.

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apparent. I have named this new species *crostotus*, from the Greek word, signifying fringed. Dr. Green's description of his species—"Clypeo, postice arcuato, angulo externo in mucronem valde producto, oculis minimis remotis, post abdomine in spinam arcuatam utrinque extenso,"—applies quite well to the *crostotus*; but this latter differs from the former in having the shield pectinate or fringed anteriorly. The spines of the shield and of the several ribs are more nearly straight. Besides the spines terminating the ribs, there are six slender teeth, similar to those of the anterior fringe, attached, not to the ribs, but to the terminal margin of the tail, four of them between the two last costal spines, at *a*, and the other two outside or anterior to the same, at *b*. Each of the costal arches is marked by two tubercles or "pimples," (one in the other species.) one on its middle, and the other at the commencement of the free spine in which each costal arch terminates. These tubercles form four rows or lines down the body, two on each lateral lobe, the inner one being in the direction of the distant eyes.—*American Jour. of Science*, &c. Cincinnati, Ohio, Dec. 9th, 1842.

MISCELLANEOUS.

*On Copal.**

THIS resin is found upon the coast of Africa. There are no trees in the vicinity, nor any thing to indicate that there ever were any. It is found a few inches below the surface of the ground, in beds resembling lava. The natives who gather it can give no account of its origin. When taken from the bed, the gum is covered with a black earthy substance, which nothing can remove but the strongest lye. As the only establishment known to exist for cleansing it is at Salem, Massachusetts, all the gum brought to this country is sent there. It is placed in vast vats, into which strong lye is poured, and after remaining there some days, it is removed, spread upon boards, and dried in the sun. The action of a stiff brush then removes the coating, and renders it fit for use. It is then assorted, the clear (which is the first quality) separated from the dark and spotted; and it is then packed in boxes, and sent to all parts of the world.

By removing the native coating, the gum is left with a pale gold color. But by cutting with a knife through the second coating, a brilliant surface is presented, that nearly equals the brilliancy of precious stones. In many pieces of gum insects are found, large, perfect, and beautiful; also fluids, some transparent, some colored.

Improvements for covering Metals with other Metals. By W. H. FOX TALBOT. Extract from the Specification of the Patent, which was enrolled May, 1823.

THIS is for silvering and gilding by the galvanic battery. The surfaces to be gilt are first silvered by dipping them in a weak solution of silver in hyposulphite of soda. Another method is by

* Silliman's American Journal of Science and Arts, Jan. 1843.

cleansing the surface at the pole of a battery, after which it is thrown into proper solutions of silver or gold, and thus receives a coating of one of these metals; and the process is repeated until the coat is of sufficient thickness. Chloride of gold in a solution of boracic acid is used for brass or other metal. The colour is more pleasing than when the chloride alone is used. The patentee also stains the thickening of the coat of gold or silver by voltaic action, when it is too thin by the dipping process. When articles have received a dark tint by dipping into an unsuitable solution of gold, Mr. Talbot removes that tint by dipping the articles in a very weak solution of nitrate of mercury. This process may be repeated until the required colour is obtained. *Rep. Arts.*

New Method of Tanning. By J. BORDIER. Enrolled July, 1842.

THE skins or hides, after having gone through the operations of washing, removing the hair, and swelling, are submitted to the action of such metallic, saline, and earthy substances as may be decomposed by the skins and hides, or may combine with them. The most preferable is the bibasic sulphate of sesquioxide of iron, or the basic red sulphate of iron, or the sub-sulphate of peroxide of iron. Directions are given for preparing the basic red sulphate from green copperas, which is by means of sulphuric acid and pulverized peroxide of manganese. A reddish liquor is obtained which, by dilution with water forms the tanning liquid into which the hides are to be placed. Three days for thin skins, and about eight days for ox skins, will convert them into imputrescent leather, which, after being submitted to the other well-known processes for giving tenacity, solidity, and impermeability to water, may be ready for use in fifteen days from the commencement of the first process.

Certain Improvements in making Oxides of Metals, and for separating Silver from Lead; also, for making White Lead, Sugar-of-Lead, &c. By JOHN MULLINS, Surgeon, Battersea.

1. The oxidation of lead is forwarded by passing air or oxygen gas through the metal whilst in a state of fusion, the oxide being skimmed off the surface as soon as a sufficient crust is formed. The remaining lead, if silver be present, is easily separated.

2. The oxide of lead is transformed into white lead by exposing it upon trays, to an atmosphere of the vapours of vinegar and carbonic acid gas, in a suitable room. The layers of oxide are one inch thick, and moistened with water, and the trays are supported one above another, with suitable intervening spaces for the circulation of the gaseous matters, which are introduced from tubes from their respective generators, or stills. Another mode of making white lead is, by exposing the acetate, made from the oxide, to an atmosphere of carbonic acid gas. The acetate is dissolved in water, and suspended, by means of sponges, in earthenware jars, and thus exposes a great surface to the acid gas. The sponges are con-

stantly supplied, from vessels placed above, through the medium of worsted strings by which the sponges are suspended. Mr. Mullins also claims a method of cleansing metals from iron, by means of magnets. A method some hundreds of years older than Mr. Mullins.

PREPARATION OF FRESHWATER SHELLS FOR THE CABINET.—"It is well known that these shells are composed of animal matter and carbonate of lime, thinly laminated. Many of them are more or less covered with mucus, lime, clay, and oxide of iron, sometimes indurated, so as to require a steel instrument to remove it. Hence the first operation is to remove this extraneous matter by hand brushes, and then with dilute muriatic acid to remove the free lime and accidental colours; then, after a thorough rinsing, and as soon as the water has dried from the surface, saturate the shells with the finest spermaceti oil, which should be left on them for several months if convenient, but wiped from them as clean as possible with a woollen cloth before putting them in the cabinet. They will then feel like steatite, and exhibit a transparency and beauty which I could not obtain in any other way. Shells which have once been exposed to the air, without the animal, and have become thoroughly dry, can never be restored to their primitive beauty, because the water of the animal matter in them has evaporated. They become opaque, and a slow decomposition, like that of salts, takes place, by the evaporation of the water of crystallization; but the oil taking the place of the water, as the latter evaporates, increases the transparency of the shell, as it does that of paper, and the superfluous oil may be so effectually removed at the proper time, that the shells will not soil the fingers or smell unpleasantly; but any considerable exposure to the air and light will soon injure their appearance."

DR. ENGELMANN'S PROCESS FOR SEPARATING GOLD OR SILVER FROM LEAD.—Take a few grains of bone ash, make it into a paste with a little saliva, spread it about one line thick on a piece of charcoal, and make a shallow impression in it, to receive the globule of metal. Expose it to the heat of the blowpipe, so as to burn it white and hard, and then melt the globule of the alloy on it, and keep it in a constant red heat, till the lead is all oxidized.

The advantages of the bone ash over the mica are manifold. 1. It is easier to be obtained, and every where the operator can prepare a little if he should not be supplied with it. 2. The metal will remain in the concavity of the bone ash paste, and not be liable to run down and be lost, as on the mica. 3. It is never necessary to change the material; the bone ash absorbs the litharge which collects on the mica, and impedes the process, so that the remaining metallic globule has to be transferred to a fresh slip of mica. 4. The colour of the paste, after the operation is finished, gives an indication as to the nature of some impurities of the metal; lead alone makes it appear yellow; a small proportion of copper changes this yellow colour to greenish.

Method of Obtaining pure Silver, either in the Metallic State or in the form of Oxide. By W. GREGORY, M.D., F.R.S.E.

THE Chemist, as well as the Metallurgist, has frequent occasion to purify silver, especially from copper, which is dissolved along with it by nitric acid, the proper solvent of silver. By converting the silver into the insoluble chloride, it is effectually purified from copper as well as from all other metals, the chlorides of which are soluble. But here the difficulty begins; the chloride of silver is a very unmanageable product, at least in the moist way. It is true, that if placed in water, acidulated with hydro-chloric acid, in contact with zinc or iron, the chloride of silver is reduced. But the process is tedious, seldom complete, and in the end unsatisfactory, for some zinc adheres to the reduced silver, so that it is not removed by digestion with moderately strong hydrochloric acid. This is proved by the action of ammonia, which extracts a good deal of oxide of zinc. Moreover, the zinc or iron is hardly ever pure, and its impurities, arsenic, carbon, and perhaps also copper and tin, remain with the silver. I have never got from silver thus reduced a colourless solution of nitrate.

After describing the objections which attach to the different methods which have hitherto been adopted for obtaining the silver in a state of purity, the author says,

The following method appears to me the most advantageous:—

The cupreous solution of silver is precipitated by common salt, while hot, and the chloride of silver well washed by decantation with hot water. It should also be broken down with a spatula of platinum, or a glass rod, during the washing; but not ground in a mortar, which causes it to cake, and impedes the action of the potash. The chloride, *while still moist*, is covered to about half an inch with a solution of caustic potash, specific gravity 1.25 at least, and then boiled. During the boiling, which is best performed in a capsule of clean iron, silver, or platinum, the chloride is to be well stirred, in order to bruise all curdy or lumpy particles. If a small portion, taken out and washed, do not dissolve without residue in dilute nitric acid, the potash is to be decanted off, and the powder, still moist, is to be well rubbed down in a mortar, which may *now* be done with advantage. It is then returned into the capsule, and again boiled for five minutes with the same or with fresh potash. It will now dissolve entirely in nitric acid, but if not, a second grinding will infallibly succeed. It is now only necessary to wash the oxide, which is completed by decantation in a few minutes, as the powder, from its great density, sinks at once to the bottom. The first two or three washings are made with hot water, the remainder with cold water, for when the oxide is nearly washed, it rises partially to the surface with *hot water*, and thus a loss is occasioned in decanting. Of course, the whole washings (except the first, owing to the strength of the potash) may be conducted on a filter. But the powder is so fine, that probably a good deal would adhere to the paper when dry.

This oxide of silver appears in a form quite distinct from that of the oxide precipitated by potash from the nitrates, and is hitherto undescribed. It is very dense, homogeneous, and has a pure black colour, which has, if anything, a tint of blue, whereas the common oxide is bulky, far less dense, and of a greyish brown colour. They appear, however, to be chemically identical. Not having a microscope, I have not studied their physical characters minutely, but I suspect from its aspect in the liquid in which it is formed, that the new oxide is crystalline.

It is obvious, that the above process furnishes an easy method of procuring a very pure oxide of silver, and of course the action of heat gives us the silver in the state of metal. It is, I conceive, applicable both to the manufacture of nitrate (in a state of absolute purity) and to the metallurgic process for obtaining pure silver. For both objects, it is a matter of no consequence if some chloride should have escaped the action of the alkali. This chloride is left undissolved by the nitric acid, and is separated by filtration, while if the oxide (not quite free from chloride) be mixed with a little nitre or carbonate of potash, and fused, the whole silver is obtained with the utmost facility. In order to give an idea of the ease with which the whole is performed, I may mention, that I dissolved a half-crown, and obtained the whole of the silver it contained, within a very trifling fraction (chiefly decanted in the *first* washing of the chloride, *but not lost*) by the above process, *within two hours*, in a fused state. The silver was quite pure. There is no doubt that to Chemists, also, an easy method of obtaining quickly, pure oxide of silver, in a form much less hygrometric than the usual one, will be acceptable.

It is particularly to be noticed, that if the chloride *have once been dried*, it is with great difficulty decomposed, even by a long boiling with potash.—*Philosophical Magazine*.

A cargo of naptha, which arrived at Liverpool last autumn, being suspected to be a spurious article, was detained by the custom-house officers, who employed Dr. Ure to analyze it; the result of which was, that this "pretended naptha" consists of 70 parts of alcohol, 14.3 overproof, (80 per cent. of proof spirit,) and 30 of pyroligneous acid. The liquid when first examined had a rank smell of pyroligneous acid, an unpleasant sourish taste, reddened litmus paper strongly, and its specific gravity was $0.942 = 19.5$ under excise proof. The acid was neutralized by a test solution of pure carbonate of potash; and indicated 1.2 per cent. of real acetic acid. Another portion of the liquor was neutralized by quick lime, and distilled by the heat of a water-bath. A spirituous liquor was obtained, of the specific gravity 0.878, equivalent in quantity to about 80 per cent. of a spirit excise proof. This being again rectified with excess of lime, lost much of its empyromatic flavour, and showed itself to be alcohol tainted with pyroligneous oil of wood vinegar, and not wood naptha.

Dr. Ure also showed the presence of this great amount of alcohol, by converting it into sulphuric ether. Another proof was by converting the alcohol into very good drinkable gin, that was much relished by good judges.

"Having terminated, for the present, the chemical proofs," says Dr. Ure, "I shall subjoin some commercial considerations upon the subject. Wood naphtha is the product of pyroligneous acid solely, from which it is obtainable in the very small proportion of less than one per cent. But that acid is made almost solely for the use of calico printers, so that where there are no great calico printing establishments, as there are none in the United States, little or no wood vinegar is prepared. Now the naphtha detained at Liverpool, amounting to about 2000 gallons, would be equivalent to at least 200,000 gallons of pyroligneous acid, a quantity certainly far greater than has been formed since the days of Elizabeth. Great Britain and France supply the American females with a profusion of printed goods of such price, beauty, and fashion, as to suppress almost entirely their small homely fabrics of the kind. If they cannot, therefore, send us naphtha, which they do not make, and could not make so cheaply as in the works of Swansea, and other parts of Wales, they can inundate England with their coarse ardent spirits in the depressed state of their distilleries, at the rate of one shilling per gallon: whereas, naphtha is worth six times that sum: and they can readily procure as much pyroligneous acid as will suffice to give their alcohol such a taste and flavour as to deceive chemists, who take *their nose for their guide*." This is the heart of the mystery.

M. MORITZ, a German chemist, being commissioned to examine two specimens of calomel, one called "Calomel prepared by steam," the other, "Calomel prepared by precipitation," found the former to contain a fifth of its weight of sulphate of barytes; the latter was *ammonia-chloride of mercury*, (white precipitate,) mixed like the former, with a fourth of its weight of sulphate of barytes,

LINSEED-MEAL.—This article in France, has been found to be adulterated with the powder of a some kind of grain, deprived of its mucilaginous principle by torrefaction, and from which the oil has been pressed, frequently mixed with bran, oatmeal, and almond-powder, with the refuse of starch-manufactories.

A SINGULAR ELECTRICAL PHENOMENON.—A short time since the attendants at Mr. Hurst's mill, Eastbourn, being warned by the appearance of an approaching storm, hastened to take in the sails. But before this work was completed, the wind blew violently, with heavy showers of sleet; and on a sudden, the sweeps of the mill became brilliantly illuminated with a blue lambent fire, which continued flitting about the frame-work for nearly ten minutes; at the end which time it gradually disappeared. Doubtless, the phenomenon was produced by a quantity of the electric fluid which settled on the mill, and there being no conductor to carry it away, it remained till it was absorbed by the atmosphere.

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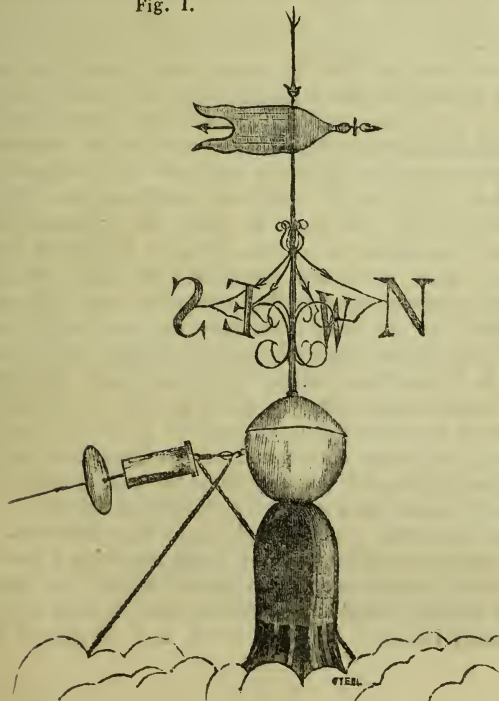
On the Construction and Management of Exploring Wires.

By W. H. WEEKES, Esq., &c., &c.

(Continued from page 106, and concluded.)

In directing attention to the view of the tower and exploring wires in their passage over it, the eye will not fail to detect two small cylindrical objects projecting immediately from the balls which give support to the vane spindles of the respective churches. These cylinders, usually denominated funnels, provide the means of insulation to the extremities of the wire, and prevent the electric fluid thereby collected from the atmosphere, from passing off down the spires by the ordinary means of conduction. In a perspective sketch it must be obvious that these funnels can only be faintly exhibited; I must, therefore, solicit particular attention to figure 1, in

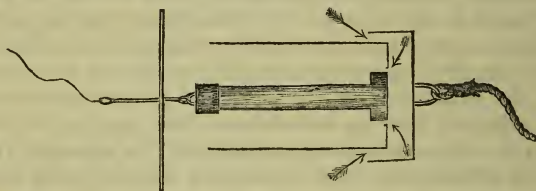
Fig. 1.



which the external character of this part of the apparatus will be more clearly perceived, together with the mode in which the circular rain-screen is attached before the open end of the funnel. In this figure are also represented the requisite appendages of staples, rope, block, and pulley, employed for the purposes of raising and lowering the insulated wires to or from their ordinary station against the balls of the churches; and furthermore, a second rope or *back-stay*, occasionally made fast below as an additional security against the strong oscillatory motion communicated to the funnel by the lashing to and fro of the wires during the prevalence of heavy gales. This portion of the out-door apparatus being one most essential to the success of the whole, it becomes important that it should be explained in detail.

In figure 2 is represented a longitudinal section of one of

Fig. 2.



the insulating funnels, which are made of stout tinned iron plate, and painted white externally, in order to repel, as much as possible, the calorific rays of the sun, which, both by their direct and radiating effects, have, in a few instances, been found adequate to the melting of a strong compound resinous cement, used for the purpose of uniting the glass and metallic portions of the funnel; thus giving rise to no small additional expense and trouble, by destroying the more fragile materials unavoidably employed in the construction of the instrument. Neither the structure nor dimensions of these insulators are precisely the same as when the apparatus was first erected a few years since, time and experience having, as I conceive, enabled me to make some few improvements; and, as a matter of course, I shall describe them as they now exist, especially as in their present form they are found to answer my most sanguine wishes. I apprehend that a moderate degree of attention to the principles manifested in the sectional sketch before mentioned, will render any letters of reference unnecessary. Each funnel, in its internal dimensions, is 5 inches in diameter, and 11 inches in length; and, into a cavity or socket of about 2 inches depth and of suitable width—projecting directly inwards from the closed end of the funnel—is firmly cemented a stout well annealed glass stick, or *cane*, as it is more frequently denominated in the manufactories. In the Broomfield funnels employed by my friend ANDREW CROSSE, Esq., there is a simple contrivance, by means of which, the glass sticks are screwed into the closed end of the instrument, from whence they may be readily removed by a few turns, and, in case of accident,

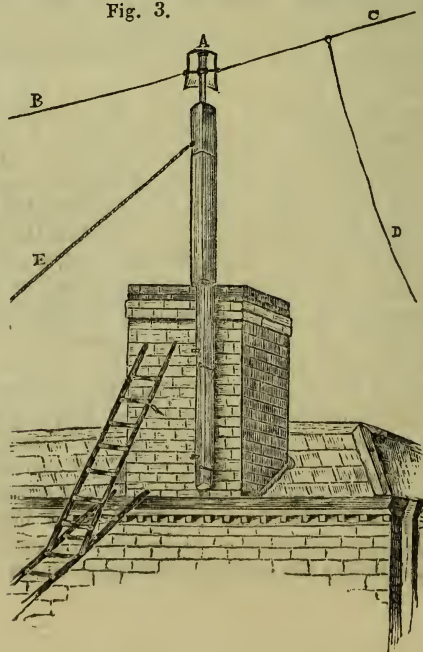
new ones substituted with little trouble. The only objection which offers itself to the general adoption of this excellent plan is, the difficulty presented in remote situations of procuring ingenious artizans to execute the requisite work of brass founding, cutting screws, &c., an advantage which, however desirable, few electricians can command. After all, if the glass sticks be securely cemented into their respective caps before the cylindrical parts of the funnel be soldered to the circular plates forming the closed ends, the matter will be easily enough managed. Plenty of room will also be found in a funnel of five inches diameter to admit the hand for the purpose of cleaning and wiping the glasses occasionally ; and in case any of the latter should be broken, a soldering tool and a very moderate share of artistical tact will remove all the impediments to putting in a new one, in the space of a few minutes. The insulating stick of which we have been treating, is about three-fourths of an inch in diameter, and of sufficient length to reach nearly to the open end of the funnel, where it is mounted, by means of the strong cement before alluded to, with a metallic cap and staple. The latter appendage receives the back of a very strong iron wire, about three-tenths of an inch in thickness, which proceeds at right angles through a disc, or circular plate of tin, placed about four inches from the mouth of the funnel. The stout wire passing through this plate is formed at its opposite extremity into a ring, to which is affixed, through the instrumentality of a copper hook, one end of the exploring wire proceeding through the air to the central station. This metallic disc is about ten inches in diameter, and as it derives free motion from the hook and staple, whatever may be the direction of the winds, it precludes the admission of snow, rain, &c., and thus by keeping the glass stick in a dry state, generally preserves an insulated condition of the wire.

I shall here take occasion to remark, that I have recently added to my insulating funnels, an appendage which I denominate an *air chamber*, a contrivance to which I was induced by the difficulty of restoring a dry state to the interior of the instrument, after the prevalence of damp weather, when the moisture that had been deposited was frequently found to linger within the cavity of the cylinder long after the sky had cleared up ; and, moreover, as some protection against the injuries inflicted upon me by extreme heats, which, as before stated, have occasionally produced a disruption of the materials, when the sun's rays have continued to operate for several hours directly and by radiation from the dome and other leaden surfaces of the churches. When this accident occurs, the whole line of wire gives way suddenly, and drops upon the roofs of the houses in an instant, causing not only vast labour to myself and several assistants, in the restoring of order, but also no small degree of consternation to certain credulous inhabitants of the town. This circumstance has more than once happened some half-hour or less previous to the commencement of a magnificent thunder storm. Let an electrician calculate the amount of disappointment !

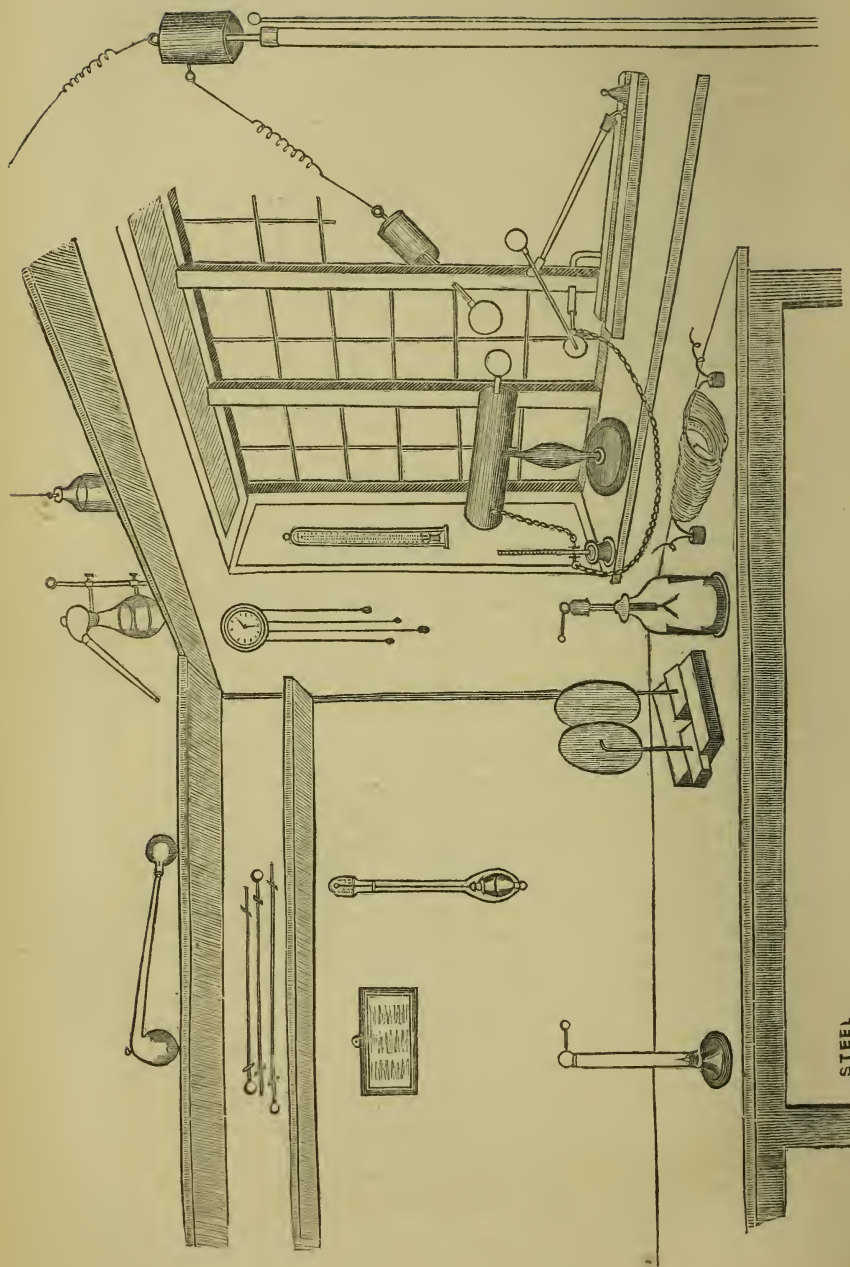
The air chamber to which allusion has been made above, surrounds the closed end of the funnel, as represented in the sectional figure, and has communication with the outer atmosphere, by means of two small tubes about three-tenths of an inch in bore, and which are placed parallel to the longitudinal axis of the cylinder, against the outside of which they are soldered. The chamber has several apertures of communication with the inner atmosphere of the funnel; these consist of holes punched through the inferior tin plate, about the diameter of the tubes before mentioned; the current of air, being supposed to enter the tubes from without, passes on in the direction of the arrow heads marked in the sketch, and thus finds its way through the chamber to the larger cavity of the cylinder. I have not only found that this simple contrivance tends materially to modify the action of the sun's rays upon the glasses and cement, but that it also promotes a more delicate state of insulation by freely ventilating the atmosphere of the funnels, after the prevalence of fogs and other damp weather.

Having described the funnels placed at each extremity of the exploring wire, I must now solicit attention to the arrangements recently adopted at the intermediate station, and at which point the wires, from the respective churches, were, in the first instance, united by a similar contrivance, capable of being lowered at pleasure by the ordinary means of a rope and pulley, until destroyed by a tremendous gale from the westward about three years since, owing to the greater strain and more frequent lashings to which the former plan subjected the apparatus. Fig. 3 represents in per-

Fig. 3.



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STEEL

The SANDWICH ELECTRICAL APPARATUS, as arranged within Mr. Weekes's Laboratory. Plate 2.—Page 117.

spective a somewhat remarkably elevated chimney attached to the house of a gentleman, whose residence adjoins my own premises. A stout post, about twelve feet in height, is placed perpendicularly against the chimney alluded to, and is secured in its position by means of several strong iron clamps driven into the brickwork. This post, independent of the glass pillar and funnel by which it is surmounted, rises to the height of about six feet above the top of the chimney. A metallic foot-stand made to fit accurately, slides over the top of the post, which it descends to the depth of sixteen inches. Into a cavity in the upper end of the foot-stand is cemented a very strong pillar of glass, sixteen inches in length, nearly three inches in diameter, and weighing about fourteen pounds. Upon the top of this pillar is placed a helmet-shaped funnel to keep off rain, &c., having within-side its upper surface a cap projecting downwards, two and a-half inches deep, for the firm reception of the end of the glass. Lower down, the pillar is further embraced by a broad belt or flat ring of metal, secured by means of four tubular braces soldered at right angles against the inner circumference of the funnel, a portion of which next the eye being supposed to be removed in the sketch for the purposes of explanation. On the external surface of the funnel, and against opposite ends of the two braces before mentioned, are also soldered two strong rings, which constitute receptacles for the copper hooks terminating the horizontal exploring wires. The several funnels are made from stout tin-plate painted white; sheet zinc, employed in my early arrangements, being found to yield to the strain of the wires, and otherwise inadequate to the purpose. Into the post, a few inches below the metallic foot-stand, are driven two iron staples, (not shown in the accompanying sketch,) whereunto the copper hooks, with their attached wires, may be removed for any incidental purpose, or during the prevalence of a heavy gale, should such a provision be thought necessary for the preservation of the apparatus; a permanent ladder, or rather a species of stair communication, having been erected against the chimney in order to facilitate such operations. A represents the glass pillar, with a section of the helmet-funnel; B and C the exploring wires passing off from the funnel in the direction of the respective churches; D the descending wire communicating with the out-door arrangements adjacent to the laboratory window, as seen in plate 1; E a strong cord, with a hook at its extremity, adapted to a staple in the side of the metallic foot-stand. This cord proceeds to a block and pulley projecting from the chimney of an opposite building, and, descending thence to the ground, affords the means of a powerful back-stay to this part of the apparatus during the prevalence of gales from the south-west, &c.

In order to render the accompanying drawing (plate 3) of the terminal arrangements of my atmospheric electrical apparatus, immediately without-side the window of the laboratory, as well as within that apartment, as intelligible as possible, I have supposed a

portion of the end-wall of the building to be removed, and thus we have obtained a diagonal perspective view of every part of the machinery essential to our present explanation. Opposite one of the main uprights of the window frame, and about four feet therefrom on the outside, is represented a stout post, about twelve feet in height, firmly planted in the ground. Upon the top of this post is placed a vertical funnel, somewhat larger than those employed at the extremities of the exploring wire, insulated and kept firm in its upright position by means of a stout glass pillar which enters a cap hid from view, but projecting perpendicularly downwards from a central point inside the crown of the instrument. With a strong smooth ring on the top of this funnel is connected a helix which forms the termination of a stout copper wire, descending from the main line, suspended over the town. This appendage, which it will readily be supposed brings down the electric fluid from the horizontal line, is left somewhat slack, in order that it may command the incidental degree of elongation occasioned by the oscillations of the exploring wire, during sudden squalls or the prevalence of long continued heavy gales. From the top of the post before mentioned descends an iron rod, three-eighths of an inch in diameter; it is affixed to the sides of the post by means of staples, and is made to terminate about a foot beneath the surface of the ground. On the top of this rod, about three inches removed from the edge of the insulated funnel, is screwed a large brass knob, which, during great electrical disturbances, provides for the escape of all superabundant fluid, and conveys it—frequently accompanied by loud cracking reports and zig-zag flashes in rapid succession—down the safety-rod into the earth. An intermediate wire helix connects the funnel last described with a second of similar character, but smaller, supported by means of a very stout glass tube, about one inch in bore, and which passes, at an angle of forty-five degrees, through the main post or wooden upright of the laboratory window frame. Within this tube is concentrically insulated, from the surrounding woodwork, a stout brass wire, not less than one-fourth of an inch in diameter, completing the communication between the funnel on the outside, and a highly polished brass globe, four inches in diameter, on the inside, which thus constitutes a terminus to the horizontal line of exploring wire. At a distance of some fifteen inches from the glass tube, serving to insulate the globular terminus, and directly beneath the same, a gun-barrel conductor passes also through the wooden upright, and, uniting itself with an iron rod of corresponding size, bent at right angles on the outside of the widow, furnishes a secure conducting medium beneath the surface of the ground, where a metallic communication is formed with a well on a distant part of the premises. This underground conductor consists of a strip of sheet lead, about two inches in diameter, buried about six inches deep in the earth. Into the hollow of the gun-barrel, on the inside

of the window, is ordinarily inserted the right-angled shank of the recipient ball, two inches in diameter, which derives motion horizontally by the sliding of the shank to and fro within the gun-barrel, and vertically from the impulse of a glass insulating lever, attached to the upright part of the shank by means of a swivel joint; while the lower end of the lever handle travels upon a small brass wheel within a narrow grooved plane of mahogany. The distance of this recipient ball from the larger one, forming the terminus, is regulated by a moveable weight placed against the wheel at the end of the insulating handle. This appendage entire is easily removed from the gun-barrel and mahogany groove, to suit the purposes of the operator in certain experiments, when it becomes necessary to connect other apparatus with the terminus of the exploring wire, and pass the electric current in another direction. The nature and use of these arrangements I have no doubt would be clearly understood by a mere glance at the accompanying drawing, but I will add, notwithstanding, that the *insulating handle* serves, moreover, to secure the operator against sundry severe shocks, arising from *lateral discharges*, which at times proceed from this part of the apparatus when powerful torrents of electric matter pass through it from the exploring wire, *although the recipient ball and its vertical connecting rod are in free communication with the ground, and also a metallic conductor laid under the ground and leading into an adjacent well.* By means of this vertical rod, ball, and lever, what we denominate *the striking distance*, is instantly regulated to facilitate the designs of experimental research, with this restriction only, that the striking distance can never be made to exceed that of the safety ball and rod from the edge of the funnel on the post outside, or more than half the diameter of the respective funnels from which the wire is suspended at its extremities. In all my experiments, however, up to the present time, the distance in question has always been found more than sufficient to produce a necessary degree of tension in the electric current proceeding from the globular terminus.

With the hope of faintly illustrating some of the most beautiful and interesting electro-chemical and other phenomena which occasionally obtain in the employment of this apparatus, I have chosen for the subject of my sketch a moment when several of the principal instruments essential to purposes of the kind, are brought into active operation. It will be sufficiently illustrative for our present purpose if I describe a single experiment as frequently performed by means of this machinery. Let us then suppose, for the occasion, that our aerial exploring wire is now transmitting a powerful and uninterrupted electrical current to the ball of the adjacent insulated conductor placed within striking distance of the terminus, and which conductor presents a surface of about 260 superficial inches, although it may be remarked that a far less amount of conducting surface would render effective the experiment to be detailed, as would, in-

deed, on most occasions, a direct communication, by means of a *small brass chain*, with the globular terminus alone; the chain being found preferable to a wire in this experiment, from the fact that it somewhat enhances the tension, and consequently the chemical intensity of the electric current. It must now be understood, that the apparatus presenting itself on the left-hand side of the conductor, and communicating therewith through the medium of the depending brass chain, consists of a stout, well annealed glass tube, about three-fourths of an inch bore, and 22 inches in length, hermetically closed at its upper extremity, graduated through two-thirds of its length into cubic inches and hundredths, and at the point where the graduated scale terminates is perforated at right angles by two large platina wires, opposed to each other, sliding horizontally through appropriate mountings, and forming rings at their outer extremities. The tube, being first charged with water, is inserted perpendicularly over a porcelain trough, nearly filled with the same description of fluid as the tube itself; and the latter, being firmly grasped by a contrivance adapted to the portable cover of the trough, is held securely in its proper situation. This instrument is, in fact, a species of eudiometer, combining, moreover, the preliminary office to set free the gaseous constituents, upon which such articles of chemical apparatus are ordinarily designed merely to operate. The necessary adjustment of instruments having been made, a discharge of sparks instantly proceeds from the globular terminus to the conductor, thence along the chain to the first platina pole, and in its energetic transit, to the opposite wire, which has been removed to an efficient striking distance from the first, rapidly converts the fluid through which it passes to a gaseous state; and when, owing to the production of an adequate aeriform volume, the water descends in the tube as low as the platina poles, an explosion of the mixed gases, caused by the electric spark, immediately obtains, and they are instantly reconverted to the state of water, in order only that they may experience repeated changes of a similar character, should the energy of the current continue.

Having brought to a conclusion the descriptive position of the present memoir, I feel induced to add a few desultory remarks which seem naturally enough to connect themselves with the subject; some of these may possibly prove interesting in a scientific consideration, and others useful to such experimentalists as may hereafter incline, with less practical advantage, to similar undertakings.

Electrical Hard Cements.—A most important acquisition to the purposes of any one engaged in atmospheric electricity, is the knowledge of a good cement wherewith to fix his insulating sticks, pillars, &c., in the various funnels incident to his several arrangements. Time and a multiplicity of trials have shown the two following compositions to be the best with which I am acquainted:—take five

parts, by weight, of black resin, one part of yellow bees' wax, two parts of the best shell-lac, and one part of red ochre or Venetian red in fine powder: let the latter be well dried over a sand-bath, or by exposure before a moderate fire. The wax and resin are to be first melted together, and the lac subsequently added; and when the solution is completed, at a temperature not much exceeding 212° , the earthy powder should be stirred in by degrees, and a moderate heat continued until all frothing ceases and the mixture becomes tranquil. For the next recipe I am indebted to my friend ANDREW CROSSE, Esq., who employs it for all out-door purposes connected with his electrical apparatus. When well made this cement is exceedingly hard and durable, but is not so easily manageable as the one above described. Take calcined plaster of Paris, powdered rosin, of each one pound; melt, mix, and add linseed oil one table spoonful. Any directions as to the mode of using these cements would probably be superfluous; but it may be worth while to mention that, notwithstanding their general excellence, I have in a few instances found the heat acquired by my funnels, in connexion with the church spires, and other elevated situations, so intense as to dislodge the glass sticks from their ordinary mountings, whereby great mischief has ensued to the apparatus. In such cases the only means of absolute security with which I am acquainted, is to surround the funnels with several folds of common white calico, made fast with twine, and easily removable at pleasure.

Carrying-out and Repairing Exploring Wires, &c.—In an open country where tall masts can be planted in the ground, or projected from the summits of high trees, the extension and raising of a line of wire, from station to station, is a work of comparative ease, when good ropes and pulleys are had recourse to for such purpose. To complete the same object, however, over groups of houses, intersected by public thoroughfares, is not always a matter of ready accomplishment. The attention of strangers, who enter the town of Sandwich, is naturally drawn to the long line of wire suspended in the air, between the principal churches, and beneath which they must necessarily pass in their perambulations through several of the main streets. After their inquiries have been satisfied as to the object of the wire itself, an almost invariable question follows:—"But how can it be got over the houses of the town and fixed at such a height in the air?" As it is possible that the solution of this question may answer other purposes than the mere gratification of curiosity, I will endeavour to answer it, first remarking that, after several unsatisfactory schemes had been tried incidental to my early operations, the following plan has been found perfectly easy and successful. A person in the first place ascends one of the church towers, armed with a long bow and suitable arrow, to which he attaches one end of a ball of strong twine; and, now, discharging his arrow into some open place in a line towards the next or intermediate station; a second person, provided also with a bow, takes up the armed arrow

and projects it in like manner over the intervening houses, and repeats his operations, if necessary, until the arrow is finally brought in the required direction to the home station, when one end of the wire, previously arranged into a loose coil for the purpose, is immediately attached to the twine, and, a concerted signal being given to the first bowman on the church tower, the exploring wire is immediately drawn over the roofs of the houses, and, together with the funnel, is raised to its ordinary locality with all desirable safety and expedition, the home end of the wire having been first made fast to a stout copper hook, which is let fall into one of the staples projecting from the helmet-funnel, a section of which is represented in fig. 3. It is almost superfluous to add that the opposite line of wire is carried out in a precisely similar manner.

Now that I am directing attention to the aerial part of our electrical machinery, a few more hints on its management may not be unacceptable. In the first place I will mention that as a security against the chafing asunder of the wires from their continuous lashing during heavy gales, I find it expedient to *double* them to the distance of some twenty or thirty feet at each extremity, where they join the eye of the rain-screen next to the respective funnels; having ascertained from experience that disruption occurs more frequently within this space than in other parts of the line. Secondly, as under any circumstances the wires will sometimes be found to give way, however great the precaution adopted, and, moreover, as coils cannot always be procured of sufficient length in the first instance, joinings must occasionally be had recourse to; hence, it may be well to mention that, after resorting to a variety of inefficient schemes, the one I shall now describe has been ultimately adopted as the most simple and effective. Whenever the joining of two wires becomes expedient, fold the respective ends backwards and parallel to the main wires, to the distance of two feet; turn the folded part back again midway, in a direction opposite the first fold, which method will give a *series of three wires*, each one foot in length, presenting, as a matter of course, a triple amount of resistance, in both directions, when the two are looped together, turned down in a spiral form and secured by the aid of pliers, as is commonly practiced by bell-hangers and other artizans, in regard to a single wire. When a line of wire gives way in the middle, or otherwise distant from either extremity, and consequently requires to be joined by the above method, it will generally be found necessary to insert a new piece between the broken ends, in order to furnish materials for the folds and looping. At certain times, but more especially at the commencement of autumn, a season when the swallow tribe begin their migration to warmer regions, vast swarms of these birds assemble in calm days upon my exploring wire, and pack themselves so densely as scarcely to leave an inch of the whole line unoccupied. Here, if no disturbance is offered, they will not unfrequently sit, as it were, in solemn deliberation during several successive hours, and, while

they present a very remarkable spectacle by their dark motionless line through the air, the accumulated weight of their legions constantly endangers my aerial conductor. I have heard my friend MR. CROSSE prefer a similar charge against the swallows; and as I imagine they entertain no respect for persons, I shall beg to add that whenever an exploring wire is thus placed in jeopardy by these little feathered people, I have recently found that if a pack-thread of sufficient length be thrown over a convenient part of the line, by the discharge of an arrow, so that both ends of the pack-thread reach the ground while the loop rests on the wire, a useful hint may readily be conveyed to the intruders by jogging the wire occasionally, when it will be seen that they perfectly comprehend the signal and lose no time in betaking themselves to flight and confusion.

Keeping Apparatus in Order.—With a view to secure the regular and successful operation of machinery, such as I have described, some pains and attention are necessary for the preservation of its general sound condition, but in no respect more so than to keep clean and dry the several glass sticks and pillars employed to insulate the exploring wire. This must occasionally be done by letting down the funnels at the ends of the line or lines, and by ascending to such as are permanent and vertical at the intermediate stations, when the respective glasses should be first well cleaned by sponging them with common oil of turpentine, and this done, they are afterwards to be rubbed perfectly dry by means of an old silk handkerchief. In attending to apparatus stretched over towns, this operation will require to be more frequently performed than in open districts, owing to the greater accumulation of carbonaceous and other matter deposited from the atmosphere within the funnels; but under any circumstances, and in any situation, some attention to the condition of the glass sticks will be found indispensable, especially after the prevalence of dense fogs. In the infancy of my arrangements I used to apply several coats of excellent transparent varnish to my several sticks and pillars of glass, under an impression that I should thereby secure their insulating properties for a greater length of time. In this I was miserably deceived; as, in the space of a month or two, I found that the insulation of my wire was lost entirely, owing, as it subsequently appeared, to moisture having penetrated the varnish and become fixed between this substance and the surface of the glass. It caused me much trouble to remove the superfluous varnish, by which means alone I could restore the insulation of my wire; but even this mishap taught me a valuable lesson, not to employ, in future, varnish of any kind, in the construction of apparatus designed for experiments in atmospheric electricity. I have lately ascertained that the vast experience of my friend MR. CROSSE, had long since induced him to give up all attempts to varnish either glass or silk for similar purposes.

Æolian Sounds proceeding from Exploring Wire, Extravagant Reports, &c.—I remember, some years since, to have met with a

notice to the effect that a certain ingenious continental experimentalist had constructed what was supposed to constitute a sort of musical weather indicator, by means of stretching a series of iron wires, of regularly increasing dimensions, horizontally over his garden; and that these wires, incidental to particular conditions of the atmosphere, were found to produce loud harmonious sounds. I cannot now refer to an authority for this statement, although I am satisfied it might have been, and probably was, in perfect accordance with the truth. Soon after the exploring wire I have been describing had been first carried over a considerable portion of the town of Sandwich, not only my own attention, but that of the inhabitants generally, was frequently attracted to the same species of music—so at least I presume—as that said to have been obtained by the philosopher alluded to in my reminiscences. I have mentioned that in the early condition of my atmospheric machinery, the wires depended from all three stations, by means of funnels held by the flexible media of ropes drawn over pulleys, and were managed, as they still continue to be, at their extremities, which terminate against the churches. The preservation, however, of my apparatus required that the intermediate station should present a more rigid and permanent degree of resistance to the strain and lashing of the wires during the prevalence of high winds, and this having been accomplished, I deem it necessary to add that the occurrence of my aerial music has been much less frequent and remarkable than formerly. One or two extracts from my early memoranda on this subject may not prove uninteresting.

“1840. May 27th.—At times during the preceding night a peculiar loud murmuring sound, forcibly reminding one of the distant strains of an *Æolian* harp, has proceeded from the horizontal wire. The last three days have been characterized by a predominance of high winds from the south-west, west, and north-west, with light haze, fleecy clouds, and, occasionally, small driving rain: at times only a weak electric action has been manifested by the test instruments. The morning of this day proved finer, with few clouds at intervals; the electroscopes being freely divergent; light breezes have succeeded from the north-west, unattended by any development of electricity.”

“June 19th.—The sky, during the past day, has been overspread by *cirro-stratus*, while the electroscopes have shown a very moderate divergence occasionally; about sunset the thick veil of cloud dispersed, and with the clear sky that followed came a succession of gentle zephyrs from the south-west. The peculiar musical sounds to which allusion has been before made in this journal, are loud and frequent to-night, and may be heard at a considerable distance from the line of wire. These sounds forcibly create in the imagination an idea of the melodies which might be presumed to arise from a gigantic *Æolian* harp placed at a vast elevation in the air. They have evidently no relation to the electrical condition of the wire, for

they have occurred when the latter has been either strongly, slightly, or not at all electrified. Clear evenings, more especially from two to three hours after sunset, would seem, as far as my observation has yet extended, most favourable to the production of this aerial music, which I regret to find is heard with a kind of superstitious awe by my uninformed neighbours, who, as a matter of course, infinitely prefer their own opinions to my assurances that the sounds in question proceed from a rational cause."

I cannot forbear mentioning here, that one of the most eminent practical electricians of this or any other period, ANDREW CROSSE, Esq., of Broomfield, in Somersetshire, has lately remarked in a paper relative to his atmospheric researches, that the most exaggerated and laughably absurd reports were at one time circulated of the enormous power said to be produced by his apparatus. I believe I shall never forget the amusement created by MR. CROSSE's personal recitation of the reports in question, to which I have more than once been an attentive and gratified listener; and, as I believe it will not now be esteemed out of place, I will add that, on the first erection of my own apparatus, equally absurd and, perhaps, even more extravagant reports, were for a considerable time prevalent throughout the town and neighbourhood; nor is it without sincere regret I am compelled to state, in my own case, that the grossest species of superstition not unfrequently operated to engender personal invective, coupled with fanatical persecution, worthy only of the darkest ages recorded in the history of our race. I will not particularise that which is best forgotten; but another extract, from one of my early journals may, I think, be tolerated in illustration.

1840. *June 2nd, 10 hours 30 minutes a.m.*—The apparatus is become strongly electrified; the distant thunder increases, while the sun glows with intense heat between the opening clouds. *Eleven hours 28 minutes a.m.*—The extraordinary heat of the solar beams, directly exerted upon the cylindrical funnel attached to the ancient Saxon tower of St. Clement, has melted the cement which held the glass insulator; the latter in its descent has been shattered to atoms, while the entire south-eastern line of wire has consequently just now fallen upon the houses. People are running to me from various quarters in a state of wild excitement, many of them imagining that "the wire," to use their own form of expression, "has been cut off by a flash of lightning;" and wondering, moreover, *that I have not been cut off too*, as a just punishment for my presumption in daring to draw down fire from the clouds! Some few days after this incident, a scientific friend, resident in a neighbouring town, playfully wrote me thus:—"I am given to understand that the good people of the ancient town and port of Sandwich have seriously decided that you are addicted to very mysterious experiments and dangerous practices; and that this is the crowning of your rash schemes, forasmuch as you tempt the very lightning from

the skies ! I verily believe they think that you will one day blow the town up ; or, rather, that you will knock it down with your lightning rods and other *magical apparatus* ; and, furthermore, that it is now become high time that a period should be put to your extraordinary proceedings !” After all, I should be wanting in sincerity, were I not to acknowledge that the pleasures and advantages which have arisen to me from the erection of my electrical machinery, have far outweighed the expenses, toil, annoyance, and all other considerations incidental to the undertaking. To me it seems that a well constructed and powerful apparatus for experiments and observations in atmospheric electricity possesses a very high degree of interest ; and, from the love I bear to scientific research generally, I cannot help regretting that philosophical instruments upon this extensive scale have not been more frequently adopted, and especially that they have not been had recourse to by some of the scientific associations of this and other countries. Should this be done, and the daily phenomena correctly noted, a few years would supply a vast mass of valuable information. The scenes which present themselves at times in connection with such arrangements are of the most sublime and brilliant character. When the gathering storm cloud, pregnant with infuriated lightnings, lingers over the horizontal conductor, successive waves of electrical fire, alternating with magnificent torrents of dense sparks, possessing most astonishing intensity, rush with loud hisses or cracking reports from the in-door terminus of the apparatus. Fluids are rapidly decomposed, metals are brilliantly deflagrated, and large amounts of coated surface are repeatedly charged and discharged in the space of a few seconds. Even from ordinary rain squalls, unattended by lightning discharges, these grand results will not unfrequently obtain ; while—the machinery being duly adjusted—the experimentalist may look on or direct his operations with the same feeling of cool philosophic satisfaction as when, in watching the light and feathery aggregations of a summer cloud, he perceives his electroscopic pendula recede from each other beneath its mild and tranquil influence.

Sandwich, June 26th, 1843.

On the Great Lancashire and Cheshire Coal Fields.

By E. W. BINNEY, Esq.

[Read before the Geological Section of the British Association at Manchester, 1842.]

COMMUNICATED BY THE AUTHOR.

Introductory Remarks.

FOR years geologists have been engaged in attempting to account for the origin of that most valuable of minerals, coal. Many hypotheses have been advanced, discussed, and abandoned, their authors having been, in most cases, but little acquainted with the

details of a coal field, and having derived their knowledge from others, or from a casual visit, more frequently than from close and long observation. Indeed, any one who peruses our standard works on geology, must be surprised at the scanty information he there meets with on one of the most interesting, and certainly one of the most important branches of geology; namely, a full and minute description of our valuable coal fields. The analyses of the chemist, aided by the powers of the microscope, have fully demonstrated that coal is of vegetable origin; and the main problem now to be solved is, where did the plants, forming coal, grow. Did they flourish on some land adjoining an estuary, were periodically carried down by floods, and buried beneath its neighbouring waters? or did they grow on the places where they are now found?

In this communication it will be my object to furnish what facts I have observed, calculated to throw light upon the subject.

General Description of the Lancashire Coal Field.

The carboniferous deposit, known by the name of the Lancashire coal field, not only occupies the chief part of the southern division of the county of Lancaster, but it extends into portions of the adjoining counties of Chester, Derby, and York,—in a line from near Macclesfield to Colne about 46 miles due north and south, and from Tarbock to Todmorden about 40 miles from W. S. W. to E. N. E. The latter is its greatest width, and by far exceeds the average; for in the south part it is not a mile wide from the new red sandstone to the lower millstone grit. It commences with the lower millstone grit, and extends upwards into the limestones of Ardwick, near Manchester, now generally considered to be the highest portion of the coal field hitherto observed in England. It may conveniently be divided into three divisions; namely, (in the descending order,) 1st. The Manchester coal field, containing the limestones of Ardwick and the coals found in the isolated patch of coal measures at Clayton and Bradford, near Manchester, occupying the low tract of country adjoining the new red sandstone plains. 2nd. The middle field, comprising the thick coals of Poynton, Bredbury, Haughton, Ashton, Oldham, Middleton, Heywood, Bury, Radcliffe, Clifton, Worsley, Hulton, Wigan, Hindley, and St. Helen's, occupying the rising ground between the new red sandstone plains and the higher parts of the country, and containing the richest portion of the field. 3rd. The lower coals found in the elevated parts of the country along the sides of the penine chain and the moorlands of the northern parts of Lancashire, comprising the coals of Rainhow, Whaley Bridge, Mellor, Ludworth, Glossop, Stalybridge, Lees, Crompton, Rochdale, Todmorden, Bacup, Burnley, Haslingden, Darwen, Blackburn, Turton, Chorley, Harrock, and Newburgh, seams of no great thickness, but valuable from their quality and position, and remarkable from their adjoining strata frequently containing remains of the pecten, goniatites, posidonia, and other shells, generally considered to be of marine origin.

The total thickness of the coal measures varies in different parts of the field. In a line from Manchester through Ashton, Stalybridge, and Mottram, to the limestone shales of Hollins Brook, it is about 2000 yards in thickness, and contains 75 beds of coal, amounting altogether to 150 feet. All these seams exceed 1 foot in thickness. In a line through Worsley, Clifton, Radcliffe, Bury, Bacup, and Burnley, to the limestone shales of Pendle Hill, I have found 36 seams, containing, altogether, 93 feet of coal. Only 10 of these seams are under a foot. In both sections a great number of smaller seams are not included in the list of coals.

Several attempts have been made to lay down all the seams upon a map; but, from the frequent occurrence of drift, the variable characters of the seams, as well as the changeable nature of their accompanying strata, it is a most difficult task to perform. Probably the best plan that has been hitherto adopted, is that of MR. ELIAS HALL, in classing the coals by the gritstone rocks which enclose them. This system does very well for the lower coal field, the rocks and seams of which are pretty constant in their characters and thicknesses; but it is not so well adapted to the middle and upper fields, the strata of which divide and thin out in such a manner as to render the task of identifying them extremely difficult.

It is not intended in the present paper to go through all the details of the various strata of the Lancashire coal field; this has been previously done by MR. HEYWOOD, MR. HALL, MR. WILLIAMSON, and other authors. My object is now to lay before you certain facts that I have observed with regard to roofs, the strata immediately overlying the coals, the seams of coals, and floors, the strata immediately underlying the coals.

As to the roofs.—These are of four kinds; 1st. a fine mixture of silica and alumina, with oxide of iron and slight traces of the carbonates of iron and lime, generally known by the name of blue bind. 2nd. Sandstone. 3rd. Black shale. 4th. Bituminous schists, provincially known by the name of “black basses.” All these deposits vary at different places, even over the same coal; but the deposits themselves generally contain the same kind of fossils. The blue binds are the most frequent, and nearly always contain remains of ferns and other coal plants; *stigmariæ*, *lepidodendra*, *ulodendra*, and *sigillariæ*, and beds of the unio and other shells: the three last named plants (especially the *sigillaria*) being often found standing erect at right angles to the planes of stratification. The upper part of the field does not contain many erect specimens, the middle portion being their chief place of occurrence. The Albert and Duchess of Lancaster mines, at Pendleton; the small coals, lying under the seven feet mine, at Dixon Fold, on the line of the Bolton Railway, (so admirably described by my lamented friend, the late MR. BOWMAN,) the five feet coal, at Little Lever; the Bent mines of Alkrington and Oldham; the four feet, King, and Arley mines of Wigan, all in the middle division of the coal field; and the foot coal, in the lower di-

vision, at Quarlton, yield them. Sometimes they are found with their roots running into and reclining on the seam, and more frequently the bole of the tree resting on the coal itself, without any traces of roots appearing. The sigillariæ are by far the most common. I have only observed the lepidodendron at Dixon Fold, and the ulodendron at Standish, near Wigan. The sigillariæ in the Duchess of Lancaster mine, at Pendleton, and the small coal at Dixon Fold, afford them as abundantly as they could possibly have grown; in the first named place I observed three specimens each 24 feet high, and about 3 feet 2 inches in circumference, standing in a shaft of 11 feet in diameter: and the whole of the mine affords them in great abundance. At Dixon Fold the trees are nearly as numerous.

Roofs of Sandstone are not very common; but where they do occur the seams of coal are generally of inferior quality; the fossils which are found in them are for the most part large prostrate coal plants, sigillariæ, stigmariæ, lepidodendra, &c.

Black Shale Roofs are of frequent occurrence, and cover most of our best house fire and caking coals. They seldom contain remains of plants, but there have been one or two instances in which upright sigillariæ have been found in them. Bivalve shells occur in several. Detached scales and teeth, and fishes, are found in nearly all. In Mr. Knowles's mine at Long-lane, Bolton, and above the Black mine, Dukinfield, the *microconchus carbonarius* has been met with. Nearly all the shales lying over the coals in the lower field teem with remains of the pecten, goniatites, and posidonia, and detached scales and bones of fishes.

Shales with highly bituminous schists, (species of impure cannel,) are not of frequent occurrence. They are found over the Black mine at Peel, and the Four-feet mine at Pendleton, and both contain abundance of the remains of fishes, for the most part entire. At these places neither the remains of cypris or microconchus are met with. In the roofs of the yard and three-quarters mines of Bradford, and the roof of the thin coal found between the Ardwick limestones (the highest coal in the series) are three Black Bass soofs, where all the remains of fishes consist of detached teeth, bones, and scales, mingled amidst countless myriads of the remains of cypris and microconchus.

As to the variability of the nature of the roof and the organic remains which it contains, I may instance the feather edge coal at Birtle Dean, near Heywood, the lowest mine hitherto found worth working in the field. At this place its roof is about a yard in thickness, full of shells of the genera pecten, goniatites, and posidonia, mixed with various ferns, lepidodendra, sigillariæ, calamites, and other plants; while at Walmersly, within two miles of Birtle Dean, the same coal is found embedded in a very coarse gritstone rock, composed of quartz and water-worn garnets.

The roofs of coals all clearly show the effects of currents of water, of different velocities. 1st. The blue binds exhibit every appear-

ance of a moderate current, which, though sufficient to overthrow and bury in fine grained mud the delicate ferns and other small plants found prostrate in them, was not able to overthrow the sigillariæ, lepidodendra, ulodendra, and other great trees. For it must be remembered that all the *upright* specimens of the stems of trees found in our coal measures are, for the most part, large ones.

Sandstone roofs represent exactly such an appearance as a continuous and violent current over a tract of luxuriant vegetation would now produce, namely, prostrate trees, lying in all directions, mingled with sand. All the tender and fragile plants have disappeared, either owing to the violent current that laid them low, or the subsequent percolation of water through the porous strata.

The black shale roofs indicate a very gentle and quiet flow of water, and show every appearance of having been a long time in their formation, as the nearly total disappearance of plants, and the abundance of unio, in the upper coal measures, and of the pecten goniatites, &c., in the lower ones prove.

The black bass roofs, however, in the upper coal field, afford evidence of the longest period of time in their formation, as many of them are nearly one entire mass of the casts of cyprides, microconchi, bivalve shells, and fish bones and teeth. In the case of the bass in the Three-quarters mine at Bradford, casts of cyprides, microconchi, and fragments of fishes, appear to constitute nearly the whole mass.

On Coal and Cannel Seams.—The seams of coals may be classed into two kinds, namely *cubical*, and those whose cross cleat runs at right angles to the main cleat, and the *rhomboidal*, or those whose cross cleat makes an acute angle with the main cleat. The first named generally occur in the upper and lower portions of the field, while the latter are found chiefly in the middle. The cleat of the coal is a subject which would take up too much space to enter into it as it ought to be treated; therefore I shall, at present, merely remark that, in most cases, I have observed the main cleat of the coal to run parallel to the line of the chief fault in the neighbourhood. It had long been my opinion that the qualities of the different beds of coal were owing to the different kinds of vegetable matter of which they were composed, having observed that where great numbers of upright and prostrate sigillariæ and large trees occurred in the roofs, the coals were generally hard and swift burning, leaving a considerable amount of white ashes; whilst where caking coals occurred their roofs were comparatively free from the remains of large trees. Every person acquainted with a coal field must have observed the bright bituminous coating of coal which the epidermis of sigillariæ and other plants has been converted into. Mr. RAY has lately furnished me with a specimen of an upright sigillaria, found standing in the seam of the Albert mine at Pendleton. Now, the outside of this specimen is of beautiful bright bituminous coal, whilst the inside is of a hard or bony character, thus clearly showing

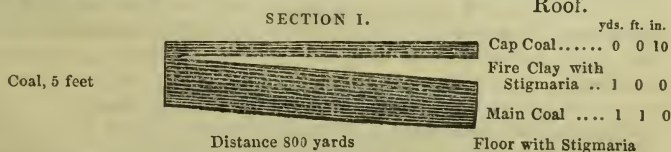
that different parts of the same tree would produce different coals. The beds of cannel are generally found on the top of the coal, and nearly always contain remains of fishes, often entire, and bivalve shells, but no traces of the cypris or microconchus have as yet been met with; rarely any leaves or stems of plants, while the upper portions of the coal seams frequently exhibit traces of plants chiefly sigillariæ, stigmariæ, lepidodendra, and calamites, and the lower parts isolated patches of pulverulent carbonaceous matter, displaying fibrous structure, but no remains of fishes or shells. Mr. SCHOFIELD gave me particulars of a singular change he once observed in a seam of coal at Oldham, where the coal under an upright stem of sigillaria was converted into good cannel. In one instance, at the New Pit of the Pendleton Coal Company, in the six feet seam several rounded stones of a fine siliceous grit were found in the coal; but as they occurred near to the great fault of 1000 yards it is probably doubtful whether they had not fallen into some of the crevices formed during the great dislocation of the strata there.

The seams of coal are either simple or compound, the former continuing undivided over large tracts of country, while the latter split and divide into several distinct seams. The first named generally occur in the lower portion of the field, and the latter in the middle and upper parts. It is owing to this fact that the thick coals of Clifton and Radcliffe cannot be well identified with the thinner and more numerous seams of Oldham, Ashton, and Bredbury, the former having a decided tendency to divide into separate beds towards the east and south-east. Whenever a parting in the coal is observed it generally indicates that the seam will separate into two distinct beds. There are many instances to prove this rule, but the one which I have most carefully investigated, and which I shall here describe, is the Four-feet mine at Pendleton. In Mr. Andrew Knowles's colliery the seam is on the north-west, one undivided seam of five feet in thickness, but as you proceed towards the south east a thin bed of fine clay, full of *stigmara ficoides*, makes its appearance, and as it is examined in Mr. Fitzgerald's pit to the south east it gradually thickens until at the extreme boundary of the last named colliery, a distance of near 100 yards from the point first observed, it has increased to three feet, thereby forming two distinct seams of coal. (See section I.)

SECTION shewing the division in the Four-feet Coal at Pendleton.

Mr. Knowles's Pit.

Mr. Fitzgerald's Pit.
Roof.

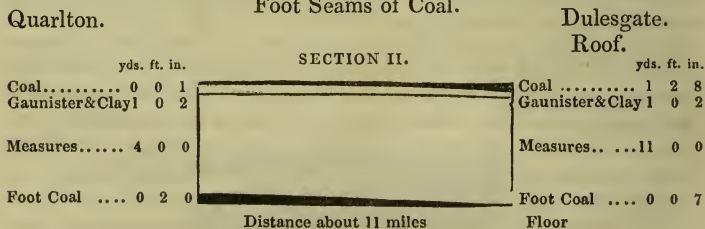


The two Bent mines were worked together by Mr. Thomas Livesey,
M 2

at Alkrington, while at Oldham, within a distance of about three miles, they are found 10 yards apart. The Cannel, and the King Coal and other mines, at Wigan, split and divide towards the east and south-east.

On the thickening and thinning of the Seams of Coal.—Independently of the tendency to divide, as last noticed, many seams diminish from a very considerable thickness to mere points. I have observed this chiefly in the lower division of the coal field, in the simple seams, though there is no doubt but that it does occur to a less extent in the middle and upper divisions, and in the compound seams. In all the six beds that have been worked in the lower series of coals there is decisive evidence of it. The examples here adduced in support of this assertion are two well known seams, namely, the caking coals, Rochdale, and the foot mines; these are taken from the facilities which the elevated tracts of country, free from drift, afford of their being carefully examined, as well as the remarkable character of the floor of the first named seam. This coal is known by many various names, in different parts of the country. It is the Parson's mine of Stalybridge, the Rochdale coal of Rochdale, and the Mountain and Rabbit seam of other parts, but it can be easily identified at every place from the singular nature of the floor, which is not composed as usual of the soft fire clay or warrant earth, that so generally prevails, but consists of a hard fine grained siliceous sandstone, full of the *stigmaria ficoides* named Gannister, well known from its excellent quality as a material for making roads. From this circumstance I have called it the "Gannister" coal. At Dulesgate, near Todmorden; it is 5 feet 8 inches in thickness, and the foot coal about 14 yards below it is 7 inches thick; I have traced them both over a considerable extent of country, as they run under and on the sides of the hills from two points, in a straight line distant about eleven miles from each other, namely, from Dulesgate to Quarlton, and find that the first named gradually diminishes in thickness from 5 feet 8 inches to 1 inch, whilst the last named increases from 8 inches to 2 feet, the floors in each case continuing the same.*

SECTION 2, showing the alteration in thickness of Gaunister and Quarlton. Foot Seams of Coal.



*The alteration in thickness in different directions of each of these two seams of coal presents a strong argument against the vegetable matter forming them being drifted into the position where it is now found, without assuming that the currents of water flowed from two totally different points. It also proves the occurrence of slight subsidences over small areas of the earth's surface.

On the Floors.—This deposit is always carefully noticed by practical miners, who believe that where a thin seam is found on a good thick argillaceous floor full of *stigmariæ*, it is certain to become, if followed, a workable coal. These floors are of three kinds, the fire clay, the warrant (a clay mixed with a larger amount of silica), and the rock floors. The first are the most abundant; the second frequent, and the last I have found only in two instances, namely, the Feather edge coal at Walmersly and Fecit having a rough quartzose sandstone, and the Gannister lately noticed. The last named is merely a fine grained admixture of silica and alumina, varying from 8 inches to 2 feet in thickness, always graduating into a fine clay at its bottom. All the seams, with the exception of the Feather edge coal, yield the *stigmaria ficoides** in their floors. I find this to prevail from the thin coal lying among the limestones of Ardwick down to the two beds found between the millstone grits of Gauxholme, near Todmorden, a distance of nearly 1,600 yards in perpendicular thickness. In the Manchester coal field all the fifteen floors afford it. In the middle and lower division sixty-nine different floors at least yield it. The plant generally has its leaves attached to it, and in all instances of true floors (those at the bottom of the seam,) it occurs without any other plants being mixed with it. This absence of other plants, and the constant characters of the floors, seem to indicate that all these numerous deposits were formed nearly under similar conditions. Their constituents show that they have been suspended in water, and very quietly deposited from that medium. In conjunction with my friend MR. J. E. BOWMAN, I have examined several specimens of the floors under the microscope, but have not yet detected any of the siliceous coverings of infusoria which have been found so abundantly in the muddy deposits of our present rivers and estuaries, by Professor EHRENBERG. MR. JOHN LEIGH has kindly assisted me in examining the chemical composition of coal floors, and from the specimens we have examined they appear to consist of silica 54, alumina 38, oxide of iron 1, water 7, total 100, a strong clay soil well calculated for the growth of the vast mass of vegetable matter required for the formation of our seams of coal. The absence of alkalies from these deposits is what might be expected from the exhausting properties of plants, and seems to strengthen the supposition that they have formerly afforded nourishment to the luxuriant vegetation now forming coal. The presence of the remains of bivalve shells and fishes in cannel beds satisfactorily prove that they were formed under water; but in the Lancashire coal field we have as yet found no remains either of fishes or shells in the seams of coal, nor is there any indication, either by the admixture of sand or silt in them, to show that they were drifted into the places where they

* It is now pretty certain that the *sigillaria* and the *stigmaria* are one plant, the former being the stem, the latter its roots. The upright tree found in Mr. Littler's white grit quarry, St. Helen's, is of the same species as the celebrated trees at Dixon Fold, and it has most certainly *stigmariæ* for roots. [E. W. B.]

are now found by rapid currents of water. In most cases we have seen that the floors and roofs give no evidence of strong currents of water, such as would have been required to transport the large stigmaria and sigillariæ respectively found in them. The occurrence of forests of large trees standing upright on the coals, the pure vegetable matter of the coals themselves, with scarce any admixture of foreign ingredients found lying upon a rich alluvial deposit of mud, apparently well adapted for the production of luxuriant crops of vegetation, seem to prove that the vegetable matter forming coal (in most cases) grew upon the spots where it is now found; whilst the splitting and alteration in the thicknesses of the coals themselves, show that the surface of the earth was most probably, at that remote age, subject to frequent subsidences.

On Caffeine, Theine, Theobromine, &c. Their Chemical Characters, and probable Physiological Action. By ALFRED BARING GARROD, Esq., M.B., Associate of the Chemical Society.

WITHIN the last few years, peculiar azotized substances, neutral in most of their properties, and having a remarkable composition, have been found in coffee, tea, the cocoa plant, and in some other vegetable bodies. The discovery of these did not excite much interest at the time, and their action on the body was supposed to be little or nothing, as they do not give to the alimentary substances, from which they are derived, their peculiar properties; but very recently, since the publication of the Animal Chemistry and Physiology by Professor Liebig, they have acquired great interest, for he has endeavoured to show, that probably man, in a state of civilization, may require such substances to assist in the formation of a secretion most important to animal life—the bile.

I shall, therefore, this evening, give you an account of these substances, including their mode of preparation, and their chemical and physical properties, and then endeavour to illustrate, by comparing their composition with that of the bile, how they may prove of use in the animal economy.

1. *Caffeine*.—This substance was discovered by Runge, in 1820. Coffee contains, besides this peculiar body, many other soluble substances, as sugar, a peculiar acid called the tannin of coffee, another acid named the caffeic or caffeic acid, also colouring and extractive matters, and a little volatile oil. To obtain caffeine, several methods have been employed; perhaps the best is to precipitate the tannin and caffeic acid by means of the subacetate of lead, from a strong decoction of raw coffee, then to digest it on freshly precipitated oxide of lead, which neutralizes any acetic acid which is set free, and causes a still further precipitate after the subacetate has ceased to do so. Any lead remaining is to be thrown down by passing sulphuretted hydrogen through the solution, and the filtered

fluid by evaporation gives beautiful crystals of caffeine, which, if coloured, can be obtained quite pure by a second crystallization and the use of a little animal charcoal.

Another mode of preparation is that of Robiquet, who precipitates the decoction of coffee by means of an infusion of nutgalls, dissolving the precipitate in alcohol, then mixing the solution with magnesia, which combines and forms an insoluble compound with the tannic acid, and leaves the caffeine in solution, from which it is obtained by evaporation. The first of these methods is preferable in this country, because it does not require the use of alcohol.

Caffeine was at first considered to be a vegetable alkali, but it is found to possess neither basic nor acid properties, its solution does not affect either red or blue litmus paper; it is not very soluble in cold water; one part of caffeine requires fifty parts of water to dissolve it, neither is it very soluble in cold alcohol, but hot water dissolves it very readily, from which it crystallizes during the cooling; acids do not alter it, but cause it to dissolve more easily than water. Its solution has a slight bitter taste. It is not precipitated by the salts of iron, copper or lead, but is precipitated by tannin.

Caffeine crystallizes in two forms, from a strong and acid solution in snow white delicate flexible needles, having a silky lustre, not unlike asbestos, but from a weak and slightly ammoniacal solution, it crystallizes in long transparent prisms, not very flexible. When heated above 212° Fahr., it loses about eight per cent. of water, and becomes opaque; at 352° Fahr. it fuses, and at a higher temperature sublimes, without leaving any residue, in needles similar in appearance to benzoic acid. The sublimed caffeine is anhydrous.* The composition of caffeine deduced from the analyses of Pfaff, Liebig, and Wöhler, is $C_8 N_2 H_5 O_2$.

In the preparation of caffeine by means of the subacetate of lead, an abundant precipitate is formed, consisting of the caffeic acid and the tannin of coffee in combination with lead. When this precipitate is washed, and suspended in water, and a stream of sulphuretted hydrogen passed through till all the lead is precipitated, and the fluid filtered and evaporated, we obtain a gummy looking mass, which consists of caffeic acid, caffiates of lime, magnesia, &c, and the tannin of coffee. To separate these, we should digest the mass in cold alcohol, which dissolves out the tannin of coffee, and leaves the caffeic acid and caffiates. Boiling water separates the caffeic acid from the caffiates, which are insoluble.

Caffeic acid reddens strongly litmus paper: when decomposed by dry distillation, it gives out the odour of roasted coffee. Caffeic acid does not alter the colour of the salts of iron, it precipitates lime and barytes water; the precipitates are soluble in nitric acid. The

* Caffeine, by the action of caustic alkalies, is resolved into carbonic acid, formic acid, and ammonia. With sulphuric and hydrochloric acids it forms crystallizable compounds. The action of fuming nitric acid and heat upon it, will be spoken of under *theine*.

composition of caffeic acid has not been accurately determined, but it is composed of carbon, hydrogen, and oxygen, without any nitrogen.

Tannin of Coffee obtained by evaporating the alcohol, which has been digested on the tanno-caffeic acid described above, forms a brown mass, soluble in water and alcohol in all proportions; it colours the salts of iron green; it is converted by nitric acid into oxalic acid.

Roasted Coffee.—Coffee, by being roasted, loses weight about 20 parts in 112; but this is chiefly water. The caffeine does not appear to be volatilized or altered, for it can be obtained from roasted coffee. In an experiment made for the purpose of determining the amount of caffeine in raw and roasted coffee—the same kind of coffee being used, I found that the roasted contained about one-sixth more than the raw. This might be accounted for by the loss of water in the roasting; but it is difficult to ascertain the exact quantity by the method used, from the sugar contained in the solution preventing the crystallization of the last portions of caffeine.

In the roasting of coffee, the caffeic acid and tannin appear to be altered, and the caffeine remains unchanged. The precipitate formed by the action of subacetate of lead in a decoction of roasted coffee, is of a dirty brown colour, instead of a bright yellow, such as is obtained from the decoction of raw coffee.

Tea.—There has been found in all kinds of tea a crystallizable substance, discovered by Oudrey, called *Theine*, which has since been found to be identical with *caffeine*.

Besides this theine or caffeine, tea contains a great number of substances, as volatile oil, a species of tannin, gum, resin, chlorophylle, &c. The volatile oil exists in greatest quantity in the green teas, it has a very powerful odour: it becomes converted into a resin by the action of the oxygen of the air.

Theine.—To obtain it from tea several methods have been employed. It can be obtained by a similar method to that described for preparing caffeine, viz., precipitating the decoction with subacetate of lead, removing excess of lead by sulphuretted hydrogen, and evaporating the filtered fluid, when crystals of theine are formed, in fine needles or in prisms, according to the state of the fluid, as to dilution, acidity, &c. Dr. Stenhouse has prepared theine by a different process, founded on the volatile property of theine: he adds acetate of lead to a strong decoction of tea, evaporates to dryness, and then sublimes from a metal bath. The *theine* is then obtained in feathery crystals, like snow, anhydrous.

Tea-leaves dried, powdered, mixed with sand, and then heated on a bath, afford crystals of *theine*, without any preparation; but the heat must be carefully regulated, or an oily pyrogenous body is formed, which dissolves the theine.

The green teas do not contain so much *theine* as the black; the average proportion is about one per cent. In some extract of tea from china, given to me by Dr. A. T. Thomson, the quantity of theine was very large.

Now, it has been found that *theine* is identical with caffeine, and, therefore, the description of the properties of caffeine applies equally to theine. A test for theine or caffeine has been proposed by Dr. Stenhouse, which is the following :

Heat quickly a portion of *theine*, with two or three times its weight in fuming nitric acid, nearly to dryness, and to the yellow substance formed add a drop or so of ammonia, an intense purple colour is produced; so the test is the same as for that of uric or lithic acids. It is very remarkable that a substance having such a peculiar composition as *theine* or *caffeine*, should be found in both tea and coffee, for these plants do not belong to the same natural families. The peculiarity of their composition consists in their containing such a large amount of nitrogen; for, with the exception of urea and theobromine (a substance from cocoa to be presently described), caffeine contains more nitrogen than any other organic substance; and there is also the same ratio between the proportion of carbon and nitrogen in it, as there is in *taurine*, a crystallizable substance obtained from bile; but we shall speak more of this presently.

Tannin from Tea.—This is contained in the precipitate formed when the subacetate of lead is added to the decoction or infusion of tea. The tannin can be separated by passing sulphuretted hydrogen through the precipitate suspended in water. It seems to resemble the quercitanic acid; it blackens the solution of salts of iron. It seems to exist in tea in combination with the theine; and the compound is much less soluble in cold than in hot water, and this seems to cause a strong infusion of tea to become turbid when cold. More tannin is said to be contained in black than in green teas, as found by Sir H. Davy, and more recently by Dr. Stenhouse, although green teas appear from taste to be more astringent.

Theine or *caffeine* has also been found in Guarana, a preparation from the *Paullinia sorbilis*, and it was called guaranine before the identity of the different substances was discovered. *Theine* has also been found in the *Ilex Paraguensis* or Paraguay tea, used by some nations in place of our common teas.

Theobroma Cocoa is the plant which yields the fruit from which cocoa and chocolate are prepared. The analysis by Schrader gave a peculiar extractive, similar to that he obtained from coffee, which extract of coffee, called also the bitter of coffee, contains the caffeine. Recently a substance has been obtained from cocoa, which has been called *Theobromine*, a crystallizable body, having a great analogy to caffeine. I do not know of any method given for its preparation; but I have been enabled to obtain it, by treating a strong decoction of the bruised nuts, after separation of the oily matter, by skimming the surface, with subacetate of lead, then filtering the fluid from the precipitate, and removing the excess of lead by sulphuretted hydrogen and evaporating; the theobromine, after a certain amount of concentration, falls, on the cooling of the liquid, in the form of a white powder, which does not appear crystalline. I have obtained it pure by subliming this white sub-

stance, when it rises in small opaque needles; but if the substance is not pure and an oily pyrogenous body rises with it, it assumes a crystalline form, in long needles.

Theobromine is a neutral body, not affecting vegetable colours. It is very insoluble in cold water, alcohol, or æther; but much soluble in the same when heated; acids also dissolve it. The composition of theobromine is $\text{C}_9 \text{N}_3 \text{H}_5 \text{O}_2$.

The common effects of infusions of tea, coffee, &c. are well known to all, viz., the antisoporific properties, and tea appears to act also as a sedative to the vascular system, when taken in large quantities, and to resemble in this respect *digitalis*. It is not my intention to dwell on these properties, but to endeavour to explain how they may act slowly, yet effectually, in a very different manner.

If we look at the composition of *caffeine*, *theobromine*, &c., and also take into consideration the fact, that they exist in tea, coffee, cocoa, and in other plants which have been used for the same purposes, by different nations, and chiefly by the sedentary, and those whose food consists in a great part of amylaceous principles, we cannot but suppose that their effects on the system must be beneficial.

For what purpose, then, can they be used? Not as nutritive bodies (strictly speaking), for their composition has no analogy to that of the albuminous or proteine class of compounds. It cannot be supposed that they act simply as tonics, though they are slightly bitter, because their composition is so very different from the simple neutral vegetable tonics, as *salicine*, *piperina*, &c., the quantity of nitrogen which they contain being so immense, much larger than in *fibrin*, or any other organic body, *urea* excepted.

Liebig supposes that they assist in the formation of the bile, and afford to that fluid one part of its composition; but in order to illustrate this, I must first give a short outline of his views with regard to the secretion, composition, and uses of the bile. Now he supposes, that the bile is one of the products produced by the decomposition of the animal tissues, and has shown, that the *carnivora*, whose food consists of substances having the same composition as their blood, that their tissues may be resolved by the action of oxygen, which is taken in during respiration, into bile and urine. The composition of the tissue having the composition of the bile and urine, *plus* a certain amount of oxygen; but in the *gramnivora*, whose food consists in a great degree of substances belonging to the starchy compounds, and whose waste of tissue and consequently production of bile by this means, must be small, from the little amount of exercise they take, these amylaceous bodies can help to the formation of that fluid by combining with certain nitrogenized bodies found in the system. This, in most cases, must be *urea*, *uric acid*, and such bodies as result from the decomposition of their tissues which takes place.

Bile, according to the researches of Demarcay and Liebig, consists essentially of choleate of soda, or of a certain organic acid (*choleic acid*) in combination with soda. This *choleic acid* probably not

being a simple substance, but, whether simple or compound, it contains all the elements of the essential organic part of the bile. It has this composition, $C_{76} H_{66} O_{22}$.

It is a substance easily decomposed, being resolved by the action of mineral acids into another acid (insoluble in water) called choloidic acid (composition= $C_2 H_{56} O_{12}$), and into ammonia, and a peculiar crystallizable substance, called taurine, which has this composition= $C_4 N_1 H_7 O_{10}$.

By alkalis (caustic) it is resolved into another acid (fatty in its nature) called cholic acid (composition= $C_{74} H_{60} O_{18}$), and into carbonic acid and ammonia, in proportion to form *urea*. Now we find that both the choloidic and cholic acids contain no nitrogen, and could be formed from the elements of starch, by the separation of part of the oxygen, and if we supposed these to unite in the system with azotized substances having a composition similar to urea or taurine, then we can easily see how bile may be formed from these starchy compounds. But we must first see whether such a separation of oxygen, from amylaceous bodies, ever does take place in the system, and then show what azotized bodies it can meet with.

We know that the animal body is capable of forming fat from such amylaceous compounds, as is seen in the fattening of animals on potatoes, &c., and in the formation of wax, by bees fed on pure honey. In the amylaceous class, the proportion of hydrogen and oxygen is such as would form water, and this is united to a certain amount of carbon; thus sugar (grape) is $C_{12} H_{14} O_{14}$, but in fat we find the oxygen in much smaller proportion, so that in the production of fat a certain amount of oxygen must be separated, the composition of all fats being between $C_{11} H_{11} O_1$ and $C_{12} H_{10} O_1$. Now, if we suppose the starch to lose a less amount of oxygen than in the formation of fat, we should have compounds formed having a composition similar to choloidic and cholic acids, and the production of such bodies is by no means improbable.

Now, in taurine we find the carbon and nitrogen to be in the ratio of 8 to 2; and the same is found in caffeine or theine, and they differ from each other only in the proportion of oxygen and hydrogen. In theobromine the carbon and nitrogen are in the ratio of 6 to 2; in urea in the ratio of 4 to 2: so if we suppose bodies formed having a composition analogous to choloidic or cholic acids, the union of these with bodies such as caffeine, theobromine, &c., would form a compound similar to the bile in composition.

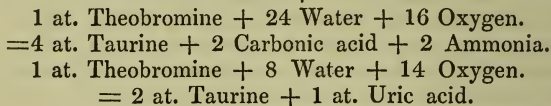
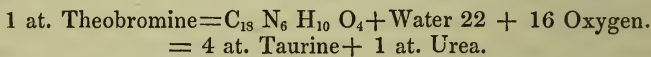
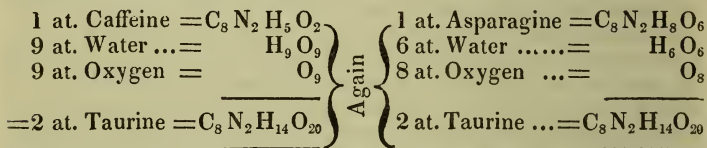
That such combinations do take place in the animal body can be proved, for when we take benzoic acid= $C_{14} H_5 O_3$, we have it eliminated from the system in the form of hippuric (as Dr. Alexander Ure has shown), and to form hippuric from benzoic acid there must be an addition of certain azotized elements to the benzoic acid; in this case, as I have shown in the Transactions of the Chemical Society, Part II., the elements are such as would form a lactate of urea, did such a compound exist, at any rate they are in the ratio to form such a compound, and both lactic acid and urea exist in the

body. Now, this case is very similar to what we may suppose to take place in the formation of the bile in graminivora.

Asparagine is also an azotized substance found in asparagus, the mallow, liquorice, &c., and has a similar composition to taurine—the composition being $C_8 N_2 H_8 O_6$.

When the waste of tissue in man or animals is small, as in an artificial state of life, then the production of such substances as urea, &c., must also be small, and the constant use of substances containing such peculiar azotized compounds as caffeine, theine, theobromine, asparagine, &c., may assist materially in the formation of the bile, and Liebig has well shown that the bile is not an excrementitious fluid, but serves some important office in the animal body: according to Liebig, it is the substance which serves for respiration; but for details on these subjects I must refer you to Liebig's work, time not allowing to do justice to the subject.

Before we leave the subject, it may be well to mention the close relation between caffeine, theine, theobromine, asparagine, and taurine, urea, and uric acids.



The quantity of caffeine or theine being so small (in tea about one per cent.) might be urged as an objection to the theory; but according to calculation, supposing $\frac{1}{100}$ part of the bile to consist of solid matter, and this solid matter to be choleic acid, which contains 3.87 per cent. of nitrogen, then it is found that $2\frac{8}{100}$ grains of caffeine would give 480 grains of bile, the nitrogen it contains in the form of taurine.

Now, from what we have seen, viz., from the identity of caffeine and theine, and the similarity in composition of theobromine and asparagine—from their being found in plants so dissimilar in a botanical point of view—from their being instinctively used by almost all nations—from their peculiar highly azotized composition—from their immediate physiological effects on the system being so little (thus differing greatly from strychnia, and such vegetable compounds as contain less nitrogen)—from their similarity in composition to taurine, I think we cannot but be inclined to consider that they have an important effect on the body, and certainly the theory of Liebig is not improbable, and deserves the greatest consideration.”—*Pharmaceutical Journal*.

On the Moon's Influence upon the Variations of the Magnetic Needle.

By C. KREIL, Adjunct of the Observatory of Prague. *Translated from the* "Astronomisch-meteorologisches." Jahrbuch für Prag. 1842. p. 94. *By* W. LETTSOM, Esq.

AMONG the heavenly bodies exerting an influence upon the natural phenomena occurring on the surface of our planet, the sun, of course, occupies the first place. Almost all the changes that take place both in the atmosphere, as well as in the external strata of the crust of the earth, are attributed either directly or indirectly to that source. Next to the sun, the moon, and that in several respects, manifests an agency in a way not to be mistaken. It is, therefore, not to be wondered at, if hopes should have been entertained that, by augmenting the frequency and the precision of our observations, traces of this influence might also be obtained in other classes of phenomena, (those connected with the atmosphere for instance,) in which it is not so distinctly pronounced. This was the case with respect to the influence which it was suspected the moon exerts upon the magnetic state of the earth. When we, however, reflect how great the distance is between the two bodies thus affecting each other, and how rapidly the magnetic force diminishes as the distance increases, we are struck at the intensity of the magnetism which would be required on the moon to produce even a very minute effect upon the earth.

When we, moreover, take into consideration how insufficient, as yet, the arrangement of most of the apparatus in which we employ for these delicate observations, and how numerous the sources of error are, we might naturally enough arrive at the conviction that any enquiries of this sort are, to say the least, beset with difficulties; that it, indeed, would be rash to enter upon them at present, and that the time was not yet come for pursuing them with success. Such, however, is the constitution of the human mind, that where there is even one single glimmering spark of the hope of success, no exertion is spared to wring from Nature one of her secrets; and, when the labour is once begun, the time and trouble expended upon it serve but as a fresh and continually urgent spur to carry that labour out.

The scientific enquirer, however,* it often happens, has no intention of entering upon any investigation of this kind, it is not comprised in the plan which he proposed to himself previous to entering on his labours; in the course of them however, it becomes evident that some phenomenon, whose existence was barely suspected before, becomes more and more marked, and then, following up this hint, he cannot but investigate the matter more thoroughly; and not unfrequently arrives thus at the most beautiful results, no ways connected

* This is an imperfect translation of NATURFORSCHEN; but I believe the term can only be rendered in English by a paraphrase about half as long as Sloane Street.—TRANSLATOR.

with those he originally had in view. The examples which the history of science offer of this, are endless; and even the inquiries into the nature of the magnetic forces present more than one instance thereof.

Thus, upwards of 40 years ago Professor Heller commenced a series of inquiries at Fulda, upon the magnetic centre of soft iron, brought into the magnetic state by the magnetism of the earth. On placing a bar of soft iron in a vertical position, we remark that it immediately becomes magnetic, and that it has imparted to it a south pole above, and a north pole below. This magnetism is strongest towards the end, and becomes weaker and weaker towards the middle of the bar. At that part where the north and south magnetism meet, there is a point where the bar exhibits no magnetic effects at all, and this point is termed the *magnetic centre*, (Anglicè, neutral point.) It is not situated at the middle of the bar, nor does it always occupy the same place, but approaches towards one end of the bar or the other, according to the different times of day, or the different seasons. These variations formed the subject of Heller's observations; and what he had in view was to arrive at the laws and the causes from which these changes depend. For this purpose, he compared them with the heights of the barometer and thermometer, and with other heads of meteorology, but was able to trace no connection between them; at last, the idea struck him accidentally to compare them with the state of the moon. It now became evident, that the distance of the magnetic centre from the south pole was uniformly considerably greater when the observation was made at the time of one of the phases than at any other period. This series of observations extended over a period of upwards of six months, or more than 25 phases. Of these, 20 bore out the rule, and only two gave a contrary result. For three phases the observations are wanting. Heller was so astonished at this result, even to him it seemed so paradoxical, that he hardly liked to make it public, although he held it to be perfectly established. "I am almost afraid," said he to a correspondent, "of stating something astrologically absurd, if I make my discovery known."

I have selected this as an example of an equally unsought for and unquestionable result, not only from its proving to us that a long while ago, and with much coarser instruments, the influence of the moon was recognized, but also because during my Milan observations I found the same thing repeated with almost undeviating regularity. I, too, originally, had no idea of bringing the moon's influence within the sphere of my observations. It forced itself upon my notice, however, in so recognizable a manner, that it would have been unpardonable negligence on my part not to follow it up more closely. At the time of my commencing my magnetic observations at Milan, the variations of horizontal intensity were obtained from the duration of the oscillations of the needle, which were noted twelve times in the course of the day, their increased or

diminished length indicating whether this force had decreased or augmented. There was now observed, independent of one regular variation,—the diurnal one,—and a second, depending on a change of temperature and the course of the seasons, a third variation in addition to these, and in which a connection with the phases of the moon could not be mistaken. *The duration of the oscillations was always shorter in winter at the time of new moon and the first quarter than during the two other phases; in summer, on the other hand, the contrary obtained.*

Shortly prior to this, the opinion,—supported on its part, too, by observation,—had been broached, that (like the earth), the sun is a body furnished with magnetic axes; and that this body varied in its action with respect to the distribution of terrestrial magnetism, as, in consequence of its rotation, it presented the one or the other of these axes to the earth. Now, as the sun completes a rotation in about twenty-five days, and therefore as seen from the earth in about twenty-seven, it must consequently, according to the above theory, bring about a regular variation upon the earth comprised within this period. And since the period indicated by the oscillations of the magnetic needle agrees pretty closely therewith, it certainly is not impossible that the cause of that variation may be situated not in the moon, but in the sun. There was also the additional circumstance of the phenomenon passing into the opposite direction in summer, and this, at first blush, appeared to be perfectly well explained by the earth in its orbit being, at different seasons, brought opposite different hemispheres of the sun.

On reflecting on the matter more maturely, however, and on taking into account the difference of the sun's period of rotation of twenty-seven days, and that of the period of the moon's phases of above twenty-nine, it is evident that the phenomenon is not to be explained by the rotation of the sun on its axis. Assuming that the sun *were* the cause; then, since in winter the shorter duration of the oscillations,—or in other words the greater intensity of the terrestrial magnetism,—coincides with the period of the new moon, it follows that the sun must, at the time of that phase, present to the earth that hemisphere which strengthens our magnetism. In the following summer the contrary must be the case. In the interval between a winter and a summer new moon there have taken place, however, very nearly six and a half rotations of the sun; consequently, at the time of new moon, the sun turns again towards the earth, the latter having also in the mean time completed half its course,—the same hemisphere which it did in winter, and the phenomena *should* coincide at the opposite seasons of the year. But as this is not the case, we cannot attribute the cause thereof to the rotation of the sun's axes, but we have to return to the moon again.

We arrive without difficulty at an explanation if we reflect that the moon must affect the horizontal needle differently, according to its different position in the heavens; and that its influence must be so much the greater the nearer it approaches to the plane in which

the needle performs its oscillations. And since this plane is the horizon, the moon, when in the zenith, or near that point, will only produce an insensible effect; whereas, when in the neighbourhood of the horizon the effect will be the most strongly marked. Now, the orbit of the moon is so constituted that in winter the moon, when new, only rises to an inconsiderable height above the horizon; while, in the summer, this body approaches the zenith at the same phase. Consequently, the new moon must exert in winter a strong, and in summer a diminished influence; and if this influence consists of an increase of terrestrial magnetism,—a diminution therefore in the duration of the oscillations of the needle,—the shorter period must in winter, and the longer in summer, correspond with new moon. And this, in fact, is in accordance with observation.

Now although the above explanation of the phenomenon in question recommends itself to us by its simplicity, it is nevertheless very desirable that it should be further confirmed in some other way. The observations themselves offer us this proof, for since the moon, according to its position, is able to modify the intensity of the horizontal force, it will, in all probability, affect its direction likewise; and we shall be able therefore to detect its influence on the changes of the variation too. It is indeed probable that those changes in intensity originate but in a corresponding change of the inclination, and that the influence of the moon on the intensity of the sum of these forces may be quite insensible. For this reason the changes in the variation were submitted to a similar investigation, and they, too, tended to confirm the moon's influence. The result was *that the variation was uniformly greater when the moon was east of the meridian, than when it had already passed it, and that it therefore acts like a body which attracts that pole of our needles which points south.* The magnetism of that half of the moon turned towards the earth, therefore, must be of a similar kind to that prevailing at the south pole of our globe, and hence it is, also, that the magnetism of the northern hemisphere is strengthened thereby.

We see from this that the observations in variation present a happy confirmation of the former results obtained from the duration of the oscillations. These results were further examined by grouping the observations in a different way, namely, according to the declination of the moon, and its greater or less distance from the earth, and uniformly found correct. One might therefore, supported by the three years' Milan observations, venture to set it up as a law that *the moon is a body subject to the magnetic force, on the face of which that is turned towards the earth, that magnetism prevails which attracts the pole of our needles pointing south, and increases the magnetism of the northern hemisphere of our earth.*

It cannot be denied that a result like this would have amply rewarded an investigation far more laborious than the present one from whence it was drawn; for it elevates magnetism, which till now was known to exist only on the earth (and even there so long but imperfectly known) to a cosmical force, whose range may be recognised

as extending at least to the nearest of the heavenly bodies, and which promises to entwine a new tie round our planetary system. But the more important a discovery is, and the greater is the joy it imparts to the breast of the discoverer; the more necessary becomes the application of the principle that doubt is the beginning of wisdom; the more he is bound to examine it in every point of view, and to test its truth in various ways.

In the present case especially, in spite of the number of the observations, (and these amounted to several thousands), and notwithstanding the extreme care with which they were made, there yet was much room for hesitation, for the influence of the moon, though plainly appreciable, was so trifling, and the apparatus used in detecting this influence, though of the best construction at present employed, was so little adapted for detecting the minute variation under investigation, that it was hardly to be expected that the results obtained should at once be accepted as true, with a perfect confidence in their correctness, and become incorporated in the body of received facts. The changes of temperature and the decrease of the magnetism of the needles are prominent sources of error, which to a considerable extent affect the duration of an oscillation. The period of the phases of the moon, comprising twenty-nine days, is tolerably long, and during that interval effects, arising from those causes, may have been brought about, and which were perhaps ascribed to the moon itself. The suite of the Milan observations includes, it is true, nearly forty of these periods, in which, therefore, those extraneous influences become for the most part eliminated. It nevertheless was desirable to introduce a shorter period also here, as had been done with the variation, where the distances of the moon from the meridian, east and west, were compared with each other, thus reducing the length of the period to a single day.

This wish was met by the observations instituted at Prague, the general purport of which was, to correct or confirm those made at Milan. They were instituted at shorter intervals than the former ones were, and the investigation into the moon's influence was not prosecuted with reference to the phases or the situation of the moon in its orbit, but to its position with respect to the place of observation. As this influence is, however, but trifling, and as it is always interfered with by conflicting actions arising from other sources, especially the far more powerful action of the sun, it became an object of importance to hit on some proceeding that would enable us, as much as possible, to exclude the action of the latter body.* In order to effect this we started from the principle that the action of the sun is manifested with great regularity, and that the cause of the irregularities observed as well in magnetic as in atmospherical phenomena must be situated much nearer to the earth, or appertain altogether to it. And this principle is justified; first, by the great

* This is described more in detail in a work under the following title, published by Ehrlich at Prague, "*Versuche, den Einfluss des mondes auf den atmosphärischen Zustand unserer Erde aus einjährigen Beobachtungen zu erkennen.*"

distance between these two heavenly bodies, in which the whole earth sinks to a point, the one-half of which is equally immersed in the sun's rays and equally exposed to its action, let that action present itself in the form of light, of heat, or of attraction. Secondly, this principle is borne out by the great regularity in the daily course of the phenomena dependent on the sun's action, as observed in the tropical countries, where this action must be most pronounced, and where, speaking generally, the atmospherical changes are so trifling that a few days are sufficient for ascertaining their daily course with great certainty. Nay, even with us their course exhibits far more anomalies in the winter months than in summer; and this could not be the case if those anomalies were to be sought for directly in the sun. Magnetic observations, it is true, are as yet confined to far too limited a portion of the earth's surface to enable us to make similar assertions respecting them with equal certainty; we know, however, that, as far as Europe is concerned, the disturbances become weaker and weaker the farther we recede from the pole. Judging from analogy, then, we may infer that within the tropics the magnetic changes take place with as much regularity as the atmospherical.

Now, if the action of the sun manifests itself in the *regular* changes occurring in the variation of the needle, the intensity of the magnetic force, or in the atmospherical phenomena, it must be expressed by the mean we obtain from a protracted series of observations instituted daily at the same hour. For instance, if the observations are continued for the space of a month, at intervals of an hour, and if at the end of the month we take the mean of the separate hourly observations, it is evident that this mean will indicate the daily course of the phenomenon with great precision, and that this result will be free from nearly the whole of those anomalies which produce such marked inequalities on distinct days.

Should the phenomenon, now, be altogether one, the daily changes of which are attributable to the sun's influence, the mean obtained, as above described, may be considered as the numerical value of that action, and by its aid the latter may be excluded from the separate observations. This is to be done by subtracting the corresponding mean from any values given by direct observation, and entering the remainder thus obtained. For it is evident, that if at the time of the observations there was no other cause but the sun's influence in operation, the result thereof, supposing the observations to be trustworthy, must equal the mean, and nothing will remain over. Should there be a remainder, however, it is the result of causes originating elsewhere, and from which the influence of the sun is already withdrawn.

By this process the most powerful of the disturbing causes, and one by which the weaker influence of the moon is entirely extinguished, is set aside, and the remainders from which that action is now thus removed indicate, when duly arranged, the influence of the moon so much the more distinctly. Other observers have, in

order, for instance, to recognize the influence of the moon on the barometer, had recourse to a twenty years' series of observations, in which however only those observations were taken into account in which the sun was always in the same position with respect to the meridian of the observer, as when on the meridian, for instance. According to the method above described the observations of a single twelve months render just as much service, inasmuch as all the observations of that period may be brought into play.

The remainders here brought into account are nevertheless only freed from one disturbing influence, though, as observed, that is the most powerful one. All the others are yet accumulated therein, and the only question remaining is, how we must set about it to render prominent, among those various influences, the one forming the subject of the present inquiry. This presents no difficulty when its action is confined to a period whose recurrence we are acquainted with, as is the case with the moon's influence. It is then simply necessary to distribute the remainders according to that period, to arrive at the conviction that in their sums or their means the effects due to all other causes, following other laws, and recurring in different periods, will be reciprocally neutralized, and this the more completely the more their periods differ from that forming the subject of our inquiry, and the more extended the series of observations is. In the case of the moon's influence, therefore, all the remainders derived from the observations made at the time of its upper transit, for example, are to be arranged in one series of which we must take the sum or the mean. And the other observations taken when the moon occupied any other particular position in the heavens are to be dealt with in like manner.

If we are unacquainted with the period within which any affecting cause is included, but if the times within which its action was in operation have been obtained by direct observation or other means, then the remainders of which we are speaking are to be arranged conformably to those observations, and we must proceed as before.

Supposing, for example, we were desirous of ascertaining the influence of the winds on the variation of the needle, the remainders would be distributed according to the winds observed, and the sum or mean be taken from every group.

What we deduce with respect to the variation from the Prague observations thus treated, is contained in the following rules.

1. *The variation is greater when the moon is situated to the east of the meridian than when it is westward.* This confirms the results already obtained at Milan.

2. *The variation is more considerable when the moon is below the horizon than when it is above it. It attains its maximum at the time of the moon's lower transit.*

3. *Although the variation, speaking generally, is less when the moon is above the horizon, yet it is wont to increase as the moon, subsequent to its rising, approaches the meridian, and after the transit it again diminishes. It is therefore least at the time of the moon's rising and setting.*

These laws, which it is hoped will be more distinctly made out by the observations and the investigations of other inquirers, point out what the daily course of the magnetic variation would be if the sun were no longer in the heavens, and the position of the needle dependent on the moon alone. We shall not further attempt to enter into the causes of the phenomenon, which is too new for us already to hope to be able to give a thorough explanation of it. No satisfactory explanation has, indeed, yet been given with regard to the changes that are attributed to the sun, and yet the fact with all its surrounding circumstances has been before us for a century.

On the Geological Structure of the Ural Mountains. By RODERICK IMPLEY MURCHISON, Esq., F.R.S., Pres. G.S., M. E. DE VERNEUIL, and Count A. VON KEYSERLING.

A short introduction explains, that although the true geological relations of the rocks which constitute these mountains were previously little known, the Russians had become well acquainted with their mineral wealth and lithological structure. The skill and energy with which the mines have been worked having been adverted to, the authors dwell with pleasure upon the facilities which the Imperial Government afforded them by the instructions conveyed to all the mining establishments by the orders of Count Cancrine and the arrangements of Gen. Tcheffkine. They also acknowledge the advantages they derived from the co-operation of many officers at the different stations or zavods, several of whom prepared maps for their use.* They further express their obligations to many individual proprietors, and notably to M. Anatole Demidof, and the Prince Butera, for their very hospitable reception at the zavods of Nijny Tagilsk and Bissersk. They then proceed to state, that without the small general map recently published by Baron A. von Humboldt and his associates, the objects of the journey could not have been so well attained. These objects were, to reunite the various fragments of the Ural chain, to show of what sedimentary masses it was originally composed, and to explain by what agency the strata have been dislocated and altered. In the latter respect they are aware that their labours have to a great extent been anticipated by the researches of Baron Humboldt, and his companions M. G. Rose and M. Ehrenbergh, as well as by their predecessors Colonel Helmersen and M. Hoffmann, and various officers of the Imperial School of Mines.

Moving in two parties and upon separate but parallel lines of research, the authors examined both flanks of the chain simultaneously, their force being brought together at the chief establishments by

* Among these officers allusion in this brief notice can only be made to those in command, viz. Gen. Glinka, Commander-in-chief at Ekaterinburg; Col. Völkner, formerly at Perm; Col. Protassof at Bogoslofsk, who first explored the districts north of that station; Col. Tchaikofski of Ekaterinburg, and Col. Galahofski of Turinsk.

mutual converging traverses; and thus, in less than three months, they acquired a general knowledge of the chain from Bogoslofsk on the north to Orsk and Orenburg on the south, a distance of about 550 miles. It is not pretended that this knowledge is precise in relation to the mineral structure of the mining tracts; as such details either have been or will be worked out by Russian engineers. The authors merely hope to have succeeded in giving a *unity of geological composition* to the chain, so that the age of the chief masses may be effectively compared with the unaltered deposits of the plains of Russia, and by this means with the geological succession of sedimentary deposits already established in Europe.

Physical Features.—Referring to Capt. Strajefski for his account of the northernmost and uncolonized part of the chain, which he explored amid great privations to 65° N. lat., the physical geography of the civilized portion is briefly sketched, and the chief altitudes, as determined by Colonel Helmersen, are given. The general bearing of the chain, as well known, trends from north to south. Ekaterinburg, the chief town, is situated on the eastern side of the only very low depression in the range, from which point this dividing crest between Europe and Asia rises both to the north and south, and attains altitudes occasionally of 2500 feet. The northern Ural, formerly occupied by Voguls, who still live in the wildernesses north of 61 degrees, is inhospitable in climate, and is chiefly occupied by dense forests, through which the rocks of the central water-shed are perceptible only at intervals. This monotony, however, is enlivened by knots of mountains which rise up on the sides of the parting ridge, and overtop it. Such are the Katch Kanar, the Pawdinskoi Kamen, near Bogoslofsk, 2784 English feet, and the Konjakofski Kamen, to the north of the same places, about 5700 feet above the sea.* Whilst the North Ural (or that north of Ekaterinburg) has one persistent direction with some lower flanking ridges parallel to the chief one, the whole not occupying a breadth of more than from 45 to 70 miles, the South Ural, *i. e.* to the south of the mountain *Jurma*,† expands to much greater width, branching off into fan-shaped ridges, which trend to the south and to the east and west of that point. In this region, however, as in the north, the water crest or *Ural-tau* preserves a north and south direction, varying in height from 1800 to 2500 feet, whilst the broken ridges on its western flanks, such as the Taganai near Zlataoust, rise to 3800 English feet, and the Iremel to about 5136 English feet above the sea.

From its configuration, and also from its latitude, the South Ural,

* This mountain was once estimated to have an altitude of 8000 or 9000 feet, but by the trigonometrical observations of Fedoroff, and the barometrical calculations of Kupffer, it has been ascertained that it cannot exceed 5280 Paris feet above the sea. It was upon this point of the range that the authors saw much snow in the month of July.

† About 3000 English feet above the sea. All these heights are taken from Colonel Helmersen and M. Hoffmann.

inhabited by Baschkirs, is infinitely more picturesque than the North Ural; but, with the exception of the environs of Miask and Zlatoust, it is much less rich in mines than the North Ural.

Geological Structure.—The Ural mountains consist of ancient sedimentary strata, which, in the central parts of the chain and on its eastern or Siberian flank, are for the most part in a highly metamorphic condition; also of various rocks of igneous or intrusive origin.

Owing to the eruption of the latter at numberless points and along great zones of fissure parallel to the axis of the chain, the ancient deposits are so dismembered and altered, that it is at intervals only they can be deciphered. The rocks are described in descending order, or from the flanks to the centre of the chain.

Carboniferous System.—By examining these mountains from their western slopes, where igneous rocks are comparatively scarce, the authors, in consequence of their knowledge of the palæozoic strata of western Europe and Russia in Europe, had no great difficulty in reading off the true order of succession on the banks of the Tchussovaya, Serebrianka, and other transversely-flowing streams. In the first place, the beds of sandstone, conglomerate, and calcareous flags alluded to in the former memoir are seen to rise from beneath the Permian deposits, and containing in some parts thin courses of coal, and in others coal-plants, Goniatites, and certain fossils, represent the upper members of the carboniferous system. These strata are succeeded by a thick formation of hard quartzose grit and sandstone, very much resembling the millstone grit of some parts of England. Beneath this is the carboniferous or mountain limestone, properly so called, of English geologists, and which is recognised by containing many of the same typical fossils as in England and other parts of Russia. Thus defined, the carboniferous system occupies, on the western side of the chain, a very wide zone, which to the south of Kongur is expanded into a large trough extending beyond the parallel of 55° N. lat., and flanked upon the west and east by upcasts of the limestone; it contains in its centre the great undulations of the grits and conglomerates just spoken of.

A third and less prolonged, but most remarkable zone of this limestone appears in four insulated hills extending north and south of Sterlitamak, and perfectly parallel to the chain. It is in the southern prolongation of this line of upheaval that the Permian red sandstones and limestones of Gre-beni and Orenburg are thrown into anticlinal positions, the axis of which is also parallel to that of the adjacent older rocks. For reasons hereafter adduced, it is inferred that this anticlinal was formed subsequent to the chief elevation of the chain.

Devonian Limestones, &c.—The Devonian rocks of the North Ural are seen on the banks of the Tchussovaya in the form of limestones, grits, and schists, which pass into the lower carboniferous limestone, the latter being always in highly inclined, sometimes in

very contorted and even inverted positions, the younger rocks dipping under the older. These Devonian limestones much resemble, in their dark colour and subcrystalline aspect, those of South Devon in England, and they contain fossils characteristic of this division both in the British Isles, in Belgium, Prussia, and the Eifel; but though perfectly identified both by position and contents with the Devonian rocks of the flat regions of Russia, the Uralian strata are as dissimilar from them in external aspect as the rocks of the same age in Devonshire are from the old red sandstone of the north of Scotland and of Herefordshire or Brecon in England. Nor are these Devonian rocks on the western flanks of the Ural separated from the lower carboniferous limestone by any band of sandstone and coal as in the northern parts of Russia in Europe, but the grey limestone of the overlying group is at once succeeded by the dark limestone of the other, both undergoing the same flexures, and both forming parts of one great palæozoic series.

In their prolongation to the south, the limestones of this Devonian group thin out and inosculate with a considerable development of red sandstone, grit, fine conglomerate, and schist, in some parts resembling the old red sandstone of the Highlands. A peculiar mineral character of these Devonian limestones is, that they retain their *black* colour even when in the state of dolomite.

Silurian Rocks.—The schists and flagstones which underlie these limestones are considered to be of Silurian age; with these strata are associated beds of limestones for the most part concretionary, and which are well developed on the banks of the Serebrianka from the zavod of Serebriansk to near its mouth. Among the predominant fossils of this group and amid numerous corals, the *Terebratula prisca* (*Atrypa affinis*, Sil. Syst.) is clustered together in great masses, as in the Sudlow rocks of England, and with it are associated the remarkable *Leptaena Uralensis* and other new species. The same descending sequence cannot be so well seen in many parts of the North Ural, as on the banks of the Serebrianka.

Immediately, however, to the east of the water-shed (viz., from Bogoslofsk to Nijny Tagilsk, and Neviansk), broken masses of limestone, insulated amid plutonic rocks, are charged with large Pentameri, closely approaching to the *Pentamerus Knightii* of the upper Silurian rocks, and associated with Orthids, Terebratula, and other fossils, which, from collections sent to him, M. de Buch has classed as Silurian forms. Although then the clear stratigraphical sequences are interrupted, there is no doubt that the equivalents, at least of the upper members of the Silurian rocks, exist in these mountains; and in tracing such into the South Ural, particularly by a transverse section from Verch-Uralsk to Sterlitamak, the authors convinced themselves, from the presence of Orthidæ, Pentameri, &c., that where not much interfered with by intrusive rocks, the central deposits of the chain (usually however in the state of slate and quartz rock) belong to the Silurian system, and probably to its lowest divisions.

The symmetry which is developed on the western side of the water-shed is almost obliterated to the east by the greater frequency of eruptive matter and the abundance of metamorphic and metalliferous rocks. Thus in passing eastwards into Siberia on any parallel, from Bogoslofsky, Nijny Tagilsk, Ekaterinburg, Miask or Verch-Uralsk, no regular succession can be traced; as large zones of igneous and crystalline rocks intervene, and thus different members of the palæozoic series are met with upon the same strike. In some spots, however, notwithstanding all this confusion, transitions can be traced from lower to higher formations. At Bogoslofsk, for example, a passage may be observed from Silurian to Devonian strata; and though all the formations are not in apposition to the east of Ekaterinburg, the section of the river Isset clearly shows, that, after various undulations, the Devonian limestones and schists on the west are succeeded on the east by true carboniferous limestone with large *Producti*, this latter deposit being in some instances based upon conglomerates and grit. Whilst this succession is exposed in a region penetrated by many points of eruptive trap and porphyry, the whole of the less altered group reposing on micaceous schists and other granitic rocks, a mass of *Pentamerus* limestone is thrown up in an insulated tract at a small distance; and as this limestone is quite dissimilar to any visible in the adjacent gorges of the Isset, where the Devonian and carboniferous limestones are fully developed, the authors conclude that it belongs to the Silurian epoch.

On the eastern flank of the Southern Ural the ancient sedimentary rocks occur in great undulations. At Troitsk in the steppes of the Kirghis, or beyond a chain of granite separated from and parallel to the Ural, Silurian and Devonian limestones occur, whilst at Cos-satchi Datchi, close to the eastern flank of the Ural, there is a small basin of palæozoic rocks, the limestone of which is proved to be true carboniferous, by containing a vast profusion of fossils, many of which are common to the Waldai limestones of Russia, and the mountain limestone of the British Isles and Belgium. In following southward the eastern slopes of the chain where they border on the river Ural, promontories of carboniferous limestone rise up in undulations, supporting troughs of the coarse carboniferous grits and conglomerates before alluded to; and on passing the axis of the chain between Orsk and Orenburg, where it dwindles to a small height, the same carboniferous group of limestones, conglomerates, and grits is thrown off to the west upon the face of the igneous rocks forming the Guberlinsk hills. In travelling westwards to Orenburgh, particularly from the limestone hills of Gourmaya, the authors found a most instructive section, developing the ascending order from the great carboniferous limestone through the overlying grits, flagstones, and calcareous grits with *Goniates*, into the beds with gypsum, which form the base of the Permian system, the whole being distinctly overlaid by conformably inclined strata of cupriferous grits, red sandstone, shale, and limestones, containing fossils of the zechstein.

Upon the eastern flanks of the Ural, on the contrary, granitic and other igneous rocks rising (as before said) to the surface, that region is entirely void of all those strata which in Russia in Europe are interposed between the carboniferous and Jurassic systems. Beds belonging to the latter system have indeed been detected at two very widely distant localities, the one in 65° N. lat. by Capt. Strajefski, the other forming a plateau in the southernmost extremity of the chain north of Orsk, where they were first observed by Col. Helmersen. It must however be observed, that the great mass of the chain is void of Jurassic strata, nor have its eastern flanks afforded any evidences of cretaceous or tertiary rocks, as identifiable by organic remains. From this last remark, the authors would except certain grits which occur in patches in the lower country of Siberia, notably at Kaltchedansk, east of Ekaterinburg. These grits, which are largely quarried for millstones, might almost be called "trachytic," as they resemble in composition some of the rough trachytes of Hungary, and like which they pass into an impure pitchstone grit. From the associated amber and beds of clay, it may, however, be inferred, that these rocks were formed under water, and that they owe the trachytic aspect to their having resulted from the detritus of the quartzose porphyries on which they repose. They are probably continuous masses of the grits described by G. Rose near Verkhoturie. Sections on the river Isset explain these phenomena.

Igneous, Metamorphic and Metalliferous Rocks.—As it formed subordinate parts only of the objects of the authors, either to study the details of the metamorphism of the sedimentary strata produced by the intrusion of igneous rocks, or the associated simple minerals, the relations of both of which have been so elaborately described by Mons. G. Rose, this portion of their memoir is chiefly confined to a sketch of some striking phenomena of this class. No true granite appears in the higher mountains, the syenite which is seen at intervals being intimately allied to greenstone; and the latter, with its various modifications, is by far the most abundant of the intrusive rocks which appear on or along the immediate flanks of the Ural ridge.* Whenever these greenstones and traps rise to the surface, the strata in their proximity are highly altered. Thus even when studied on a small scale on the western flank of the mountains, as at the baths of the Zavod of Sergiefsk, the sandstone in contact is altered into quartz rock, and the limestone, so regularly bedded and full of fossils at a little distance, is converted into an amorphous, crystalline, splintery mass, charged with cross veins, and sulphureous saline waters flow from its base, the adjacent rocks being also much impregnated with iron ore. Similar but on a far grander scale are the phenomena of intrusion and metamorphism which are presented by the central axis of the Ural, and to a less extent by all the parallel ridges which flank it on the eastern or Siberian side.

* The granitic region is in Siberia, to the east of the Ural.

The Ural-tau or crest is to a very great extent a wall of schist and quartz rock diversified by points of igneous rocks, and though of no great altitude, it is very remarkable that throughout seventeen degrees of latitude this water-shed is not broken through by any great transverse valley. The Ural-tau marks, in fact, one long line or fissure of eruption. With the exception of the gold mines near Bissersk, on its west flank, all the gold alluvia of the chain occur on its eastern flank; and when it is stated that this circumstance is connected with the fact, that all the great masses of igneous rocks have been evolved on the eastern flank, it will at once be seen (as insisted upon so well by Humboldt) that there is an intimate connexion between the eruption of plutonic rocks and the formation of the gold mines [veins?] whence the local alluvia have been derived. That this connexion exists in regard to other mineral veins, is also equally apparent in the Ural mountains; for with very rare exceptions, it is only on their eastern or eruptive side that copper veins, malachite, platinum, and magnetic iron prevail.*

Without entering into all the lithological distinctions of the North Ural, they advert specially to the occurrence in the districts of Turinsk and Nijny Tagilsk of a stratified and regularly bedded porphyry, which they compare with the "Schaaistein" of German geologists, and which on the banks of the Kakwa and east of Bogoslofsk, as in the Rhenish provinces, alternates with limestone strata of Devonian age. In the copper mines of Turinsk, the veins and masses of ore are shown to be intimately connected with the intrusion of greenstone, between a thick mass of which and the metalliferous veins is a garnet rock. This phenomenon is a counterpart to that formerly described by Professor Henslow in the Isle of Anglesea; whilst on the river Kakwa, the ordinary limestone (Devonian?) has been converted by a dyke of greenstone into white granular marble, in the same way as by the contact of syenite the lias limestone of the Isle of Sky has been changed.

The Katch-kanar mountain (lat. $58^{\circ} 44'$), which the authors visited by a little-frequented pass, is composed of augitic greenstone and magnetic iron, the latter in so hard and crystalline a state that it is not worthy of extraction and manufacture. The most productive masses of magnetic iron are at the Government establishment of Mount Blagodatski, and that of Nijny Tagilsk, belonging to M. Anatole Demidoff. In both these cases the ordinary varieties of iron occur in great masses, occasionally with chromate of iron,† in contact with rocks of igneous origin in which serpentine, compact felspar, green-

* In sketching the chief relations of the plutonic and metamorphic rocks of the North Ural, much praise is given to a detailed geological map of the environs of Bogoslofsk by Capt. Karpinski, of the School of Mines, with a copy of which the authors were furnished.

† The largest masses of chromate of iron occur in South Ural, near the mines of Polikofski, south of Miask, and from whence from 6000 to 7000 "pouds" per annum have recently been sent to Moscow.

stone, porphyry, &c., are apparent. At Nijny Tagilsk the chief intrusive rock (greenstone) is coated by prodigious masses of the iron ore, which is worked in open quarries, and is most magnetic where it is in contact with greenstone. Copper ores also abound at this spot, and some of them are associated with Silurian limestone, often highly mineralized, but in which large Pentameri and other fossils are observed; also with a bedded trappean rock or *schaalstein*, which is in parts highly cupriferous. It is from such ancient rocks that copper solutions are supposed to have flowed, in very remote periods, into the adjacent low countries on the west, then under the sea, and to have impregnated the sandstones and grits of Perm during their formation. The Malachites of this place have long been celebrated, and, from their structure as well as their position, in cavities of the rock, they are supposed to have been formed by ancient stalactitic depositions. The ores of platinum, though hitherto found in alluvia only, always occur near the protrusive igneous rocks. Magnetic iron ore and copper ore are stated to occur at many other localities, and always under similar circumstances.

Gold Ores.—Though the great supply of gold which the Ural mountains afford, is derived from alluvia, the ore has been found in veins which are slightly worked at Berosofsk, near Ekaterinburg, and were formerly near Miask. Wherever gold veins or gold alluvia have been discovered, the auriferous matter is flanked by rocks of intrusive origin, and these are very frequently serpentine. It has however been shown by Humboldt and Rose, who, in the first volume of their recent work, have described twenty-seven sites of gold alluvia in these mountains, that the auriferous detritus rests upon a great variety of rocks, viz. talcose, chloritic, siliceous, argillaceous schists, and encrinite limestone, as well as upon granite, greenstone, and serpentine, though most frequently on the last-mentioned rock. An observation also of these authors is important, as bearing upon the relative date of the origin of gold, viz., that the veins containing it have been seen by them to cut through not only the schists and the beresite (according to them a felspathic granite,) but also the serpentine; thus seeming to prove that the gold veins have resulted from one of the very last changes which have affected this region.

It is stated, that as the alluvia containing gold are purely of local origin, or derived from the adjacent hills, their accumulation can have no reference to the actual period, and present rivulets or waters, for the deposits lie at considerable heights above their beds, contain bones of mammoths, the extinct rhinoceros, and, in some instances, are even traceable over small ridges of intrusive and altered rocks from veins whence the detritus was doubtless derived, and accumulated in its present state at the period when the large mammals were destroyed. Numerous sections are given at Berosofsk Soimanofski Zavod, and notably from the environs of Miask and Cossatchi Datchi, all of which tend to establish these views, as well as those of the alteration, minerallization and crystallization of the

palæozoic strata by the intrusion of igneous matter, and prove that the alluvia were collected anterior to the existing epoch. Some of the gold alluvia are exclusively composed of carboniferous limestone replete with fossils (Cossatchi Datchi).

In concluding this sketch of the Ural mountains, the authors advert to the remarkable fact, that all the superficial detritus is local, and that no large boulders or blocks transported from afar are visible either in the chain or in the low countries on its flanks; and they also state, that they nowhere observed among the higher portions of the mountains any traces of those scratches or polishings of the rock which are common in some parts of Europe, and which are supposed to have been produced by glacial action.

Original maps and sections of the districts around the mining establishments of Bogoslofsk, Turinsk, and Blagod, at Ekaterinburg, Soimanofsk, Zlataoust, and Miask, prepared by the officers of the Imperial School of Mines, were exhibited, as well as a map of the North Ural to 65° N. lat., drawn by Strajefski, together with a most elaborate geographical map of the South Ural, executed by orders of General Perovski, under the superintendence of the officers of the staff of his government, directed by General Rakosofski. From all these documents and others published in the volumes of the "*Journal of the School of Mines*," combined with their own observations, the authors have coloured geologically the map of Humboldt, a reduction of the chief features of which will appear in a map now in progress, which will accompany their forthcoming work on Russia and the Ural mountains.

General conclusions.—In greatly extending the knowledge which they had previously acquired, the survey of last year has enabled the authors to modify their earlier views concerning the equivalents of some of the strata of Russia in Europe. With respect to their former account of the great tripartite palæozoic series of beds which cover such large portions of Northern Russia, they have nothing to retract. On the contrary, by adding to their previous lists a great number of typical organic remains well known in Western Europe, they are still more convinced of the accuracy of their first classification, and of the existence of large zones of Silurian, Devonian, and carboniferous rocks, clearly separated from each other by their order and their imbedded fossils.

The newly discovered dome of Devonian rocks in the centre of European Russia is a feature of great importance, in explaining the difference between the mineral basin to the north and that to the south of it. The carboniferous system, the most widely extended deposit of northern Russia, has now been subdivided into stages, each characterised by its fossils; and it has been clearly shown, that the most productive of the coal-bearing strata in the Russian empire, viz., those of the southern steppes, are associated with the mountain limestone; whilst the uppermost member of the system, or coal-measures, which is so rich in coal in Western Europe, if indeed it exists, is nearly unproductive in Russia.

The next group of rocks in ascending order, is that which has been elaborated in considerable detail under the name of the Permian system, and which, as already shown, is to be considered as a vastly expanded equivalent of the zechstein and associated beds of Germany and the magnesian limestone of the British Isles. This system is rendered much more important by its fossil contents in Russia than by any remains which have been discovered in it in other parts of Europe ; for not only does it contain, like the zechstein of Germany and the magnesian limestone of England, the remains of thecodont saurians and certain fishes (*Palæonisci*), but also a fauna much more copious in other classes, and a flora infinitely more rich than any which had been previously made known as pertaining to rocks of this age. This flora is shown to be of intermediate characters between that of the carboniferous system and the plants which have been published as typical of the trias.

The Permian system is also of high interest in setting before us the example of wide accumulations impregnated throughout great thicknesses with copper, and as this matter has manifestly been derived from the mineral masses of the adjacent Ural, so is it inferred that these mountains constituted dry land on which the plants in question grew, and that the latter having been washed down into these Permian deposits were there rendered the nuclei of the copper ores which are arranged around them. The thin layer of kupfer schiefer of Germany may be considered as the miniature representative of this great metalliferous deposit, whilst in it large masses of gypsum, the Permian deposits exceed even the zechstein on the south of the Hartz. The Jurassic system of Russia reposes on the Permian and older rocks without clear evidence of the existence of any part of the Triassic group, there being no traces of the muschelkalk limestone nor yet of the keuper ; and it is with doubt even that the authors refer any portion of certain red strata which partly overlie the Permian rocks to the "bunter sandstein," or new red sandstone of geologists.

True lias has not yet been seen, but the Jurassic system is clearly divisible into upper and lower formations, and is followed by the cretaceous and tertiary systems, the latter including eocene, miocene and pliocene shells, and all these groups are copiously developed and clearly recognisable by their respective mollusca.

The geological survey of the flat regions of Russia, add the authors, in affording the best proof which has yet been obtained in any part of the world of *the same extent*, that distinct forms of animal life were successively created and entombed in each succeeding deposit, has also demonstrated that the successive obliteration of these classes was not caused by the outburst of contiguous plutonic rocks or great physical disturbances of the strata ; for in this region, as large as the whole of these districts of the continent of Europe where geology has been most studied, no intrusive rocks are visible, and the wide spread formations from the Silurian to the youngest

tertiary, which must have occupied so vast a lapse of time in their accumulation, as well as the beds of retired modern seas, all repose conformably upon each other. And yet with this regular sequence throughout so vast a series and the absence of any great ruptures, the contents of each succeeding system of older strata are as clearly separable from each other as in those parts of the world where younger rocks are incumbent on the uplifted edges of those which had been previously dislocated.

But whilst they offer no traces of great and violent upheavals, the horizontal rocks of Russia bespeak most clearly that their surface has been so far acted upon by elevatory or subsiding movements, that in some tracts great thicknesses of strata are omitted. Bounded as this large geographical basin has been in remote epochs by the plutonic eruptions of Lapland and Sweden on the north, of the Ural on the east, of the granitic steppe on the south, and of the trappean rocks of Poland and Silesia on the west, it is possible, however, that the changes which were evolved in these regions may have affected and influenced the distribution of animal life in the great Muscovite depression which they surrounded. As every geological phenomenon in the strata of the plains of Russia indicates a submarine succession, so does the surface announce the same conditions. In the far northern districts the bottom of the Arctic Sea has been shown, by the presence of many existing species of shells, to have once extended over a wide tract of land, now 150 or 200 feet above the sea level; and in the south-west it is known by like proofs that the Caspian once covered still wider districts of the steppes. Again, the authors have endeavoured to show that the mammoth alluvia, the boulders of the North and the black earth of Central and southern Russia, have all been accumulated *under water*.

In reference to the question of the transport of the northern blocks, the authors conceive that their last survey has tended very materially to strengthen the opinions which they previously expressed that such materials were carried to their present positions by floating icebergs liberated from ancient glacier in Scandinavia and Lapland, at a period when Russia in Europe was submerged. The examination of the Ural has in the meantime convinced them of the utter inapplicability of a terrestrial glacial theory even to all mountainous tracts of the earth; for these mountains, the peaks of which rise to upwards of 5000 feet above the sea, though situated in so cold a climate as to be now covered with snow during eight months in the year (and some peaks are never uncovered), show none of those signs insisted on by glacialists, of their having been at any period the residence of permanent glaciers. With the total absence of such proofs, so is it a striking confirmation of the connexion between glaciers and the blocks which in Russia in Europe are supposed to have been floated from Scandinavia and Lapland, that the flanks of the Ural chain and the adjacent plains are entirely void of all such far-transported detritus.—(*Phil. Mag. and Jour. of Science.*)

On the Compound Nature of Nitrogen. By GEO. J. KNOX, Esq.*

SOON after the discoveries of the bases of the alkalies and earths by Sir Humphry Davy, the compound nature of nitrogen began to be a subject of discussion amongst chemists; but the arguments in favour of this supposition, deduced principally from the nature of the ammoniacal amalgam, led to no satisfactory physical results.

The experiments of Sir Humphry Davy on the ammoniacal nitruret of potassium, and those of Despretz and Grove on the compounds of nitrogen with iron, copper, &c., have shown that the metals singly (even when aided by the most powerful electrical induction) have not the power of decomposing nitrogen. There is one experiment, however, by Sir Humphry Davy, from which one might deduce its compound nature.

Upon heating ammonia-nitruret of potassium in an iron tube, he obtained more hydrogen, and less nitrogen, than the ammonia ought to have given.

Again: on mixing this substance with a greater portion of potassium, he obtained still more hydrogen, and less nitrogen; whereas, on heating the same substance in a tube of *platinum*, the potassium alloyed with the platinum, and the ammonia was given off almost entirely undecomposed.

How can these experiments be explained except upon the supposition that the potassium and the iron had *conjointly* decomposed the nitrogen? The latest experiments which bear upon this subject, and from which I received the idea which led me to this investigation, are those of Dr. Brown "upon the conversion of carbon into silicon," an explanation of phenomena which appears to me most unreasonable, and contrary to all chemical analogy; whilst the supposition of the carbon having reduced the nitrogen is not only a simple but an unavoidable conclusion to arrive at, if nitrogen be a compound substance. To determine, by experiment, the correctness or incorrectness of this idea, it were only necessary to reduce nitrogen by some other substance than charcoal; and should silica result from its decomposition, the problem might be considered to be solved.

Exp. I.—A considerable quantity of ammonia-nitruret of potassium was formed, by passing ammonia over potassium heated in an iron tube; the part which had not been in contact with the tube, having been examined for silica, contained none.

Exp. II.—Ammonia was passed for several hours over pure iron, heated to a dull red heat; examined for silica, it contained none.

Exp. III.—Ammonia-nitruret of potassium was heated with pure iron in an iron crucible, for one half hour, over a large Rose's lamp; the contents of the crucible, on examination, gave silicon and silica, the weight of which was not registered, as it might have been said to have derived a portion of silica from the inner surface of the crucible.

* Read at the Royal Irish Academy:

Exp. IV.—Twenty grains of ammonia-nitruet of potassium were heated with twenty grains of pure iron in the *same* iron vessel for one half hour; when treated with nitric and muriatic acids there remained insoluble a small quantity of a brownish colour, which, when fused with carbonate of potash, gave of silica 0·10. The solution, supersaturated with potash, filtered, neutralized, evaporated to dryness, gave of silica 1·450; sum total of silica 1·550.

From these experiments, together with those of Sir Humphry Davy mentioned above, one might infer that nitrogen is either a compound of silicon and hydrogen, or of silicon, hydrogen, and oxygen; to determine which, synthetically, a current of dry muriatic acid gas was passed over siliciuret of potassium (formed by heating silica with potassium), placed in a bent tube of Bohemian glass, the extremity of which dipped into a cup of mercury, lying on the bosom of a vessel filled with water. The atmospheric air had been previously expelled from the apparatus by a current of hydrogen.

The gases insoluble in water having been collected, were found, on examination, to be hydrogen and nitrogen, the relative proportions of which varied in different experiments.

In two experiments the proportions of hydrogen to nitrogen were four of the former to one of the latter.

In a third experiment, as six of hydrogen to one of nitrogen.

In a fourth, as five of hydrogen to four of nitrogen.

Magnetic-Electrical Gilding, Silvering, &c.

A MR. WOOLRICH has lately obtained a patent for electro-typing, plating, gilding, &c., by means of electric currents produced by magnetic excitation! The patent is dated Aug. 1, 1842; specification enrolled Feb. 1, 1843.

It would be impossible to know the extent of information which Mr. Woolrich had on the subject prior to his venturing his hundreds on this patent: but it is a duty incumbent on us, not only for the benefit of the public, but also for the patentee's information, in order that he may not venture on expensive law-suits, to state, that electro-typing, silvering, gilding, &c., by the very same process, was practised (not under patent) some years prior to the date of his patent.

In a paper which was read before the Royal Society, June 16th, 1836, and printed in vols. 1 and 2 of the *Annals of Electricity*, &c., it is shown that copper, tin, gold, &c., were precipitated on other metals by magnetic electrical currents. In the first part of this paper a magnetic electrical machine, *without iron armature*, is described, and also the work it would perform in this capacity; and in the second part, another machine *with iron armature*, is described, which is the first of this class, by which the various currents produced, were conducted in *one and the same direction*, independently of which contrivance, this class of electrical machines could never have been employed for depositing metals from their solutions. Hence, we may fairly conclude, that we have a *double claim* in this process.

In March, 1842, five months prior to the August patent, we published a shilling pamphlet on the subject of electro-gilding and silvering, in which, at page 47, we expressly state that we have employed magnetic-electricity "to great advantage, in silvering, gilding, and platinizing various metals of inferior value," and that we "have produced good electro-types, on a small scale, by its employment."—EDIT.

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On the Fundamental Origin of Thermo-electricity.

By BARON FABIAN, VON WREDE.

[From Poggendorff's Annalen, Vol. 55, p. 175.]*

BEQUEREL announced, several years since, that the order in which metals assume positive electricity, when heated in contact with each other, is the same that obtains with respect to their propagation of radiant heat. And he consequently infers that the thermo-electric properties of bodies may possibly be in a direct ratio to their powers of radiation.

Supposing this conjecture to be correct, we must assume that on the contact of two metals there is a radiation of heat from the surfaces in contact, as there is in the air, and that the direction and the intensity of the current depend on the difference between their powers of radiation.

Now, since iron, as is well known, possesses the property, when brought into contact with almost all other metals, of becoming strongly *positive* at low temperatures, but at high temperatures, on the other hand, strongly *negative*; it follows that if the view in question is correct, its power of radiation must at low temperatures be greater, and at high temperatures less, than that of all the other metals. In order to put this to the test with respect to copper, two crucibles were made, as similar as possible, one of iron and one of copper. These crucibles were filled with equal weights of copper filings, and the point of junction of a thermo-electric pair consisting of a copper and a silver wire was inserted therein.

The temperature of the crucibles was now raised to a red heat, *Annals of Philosophical Discovery, &c.—No. 4.—Oct., 1843.* O

* Translated by Mr. W. Lettsom.

and they were then allowed to cool, the position of a galvanometer in connection with the thermo-electric pair being uninterruptedly observed at regular intervals, agreeing with the oscillations of the instrument.* The difference between every two successive indications of the instrument afforded a relative measure of the rapidity of the cooling of the crucibles, and consequently also of the radiating power of iron and of copper at the different temperatures indicated by the galvanometer. It was now seen, beyond the possibility of error, that the iron at first, at an elevated temperature possessed a *lower* radiating power than copper, whereas towards the close of the experiment, or in other words at a lower temperature, its radiating power was considerably *greater* than that of copper.

Subsequently, the junction of a thermo-electric pair, consisting of iron and copper, was inserted in the copper crucible, and the indications of the galvanometer were observed as before, from a red heat downwards. In order to give these results a form in which they might be compared with the former ones, the position of the galvanometer was separately observed for each of the two thermo-electric circuits, at the temperature of melting lead. On the results of all the experiments being compared together, it appeared, 1st. That in the last experiment the galvanometer stood at zero at almost exactly the same temperature at which iron and copper possess an equal radiating power; and 2ndly. That the differences obtained in the first experiments, in the rate of cooling of the iron and copper, were very nearly proportional to the indications of the galvanometer as they varied with the temperature in the last experiment, so that on multiplying the first results by a certain constant, the products represent very accurately the indication of the galvanometer at the corresponding temperature in the last experiment. From all this we appear to be perfectly justified in inferring that the conjecture of Becquerel is correct. (Preliminary Notice from the *Forhandlinger ved de skandin. Naturforsk. an det Möde, &c., &c.*, 1840.)

Poggendorff on the Thermo-electric Counter-current.

[From Poggendorff's *Annalen*, vol. 58, p. 76.]

WE are well aware, from the fact discovered by Peltier, that a hydro-electric current, traversing a compound arc, consisting of two metals, always produces in that arc a thermo-electric current in an opposite direction. (*Annalen* 43, p. 328.) The following is a simple method of rendering this fact evident, the truth of which has been borne out by the experiments of Moser, Lenz, and others. Let one end of a good thermo-pile be clamped to a voltaic battery and to a galvanometer at the same time. On now connecting tran-

* This galvanometer is described in Poggendorff's *Annalen*, vol. 55, p. 604, and there is a translation of that article in the *Phil. Mag.* vol. 20, p. 379, May, 1842.

siently, even only for an instant, the other pole with the galvanic battery, and immediately afterwards with the galvanometer, the needle of that instrument will be strongly deflected, owing to the change of the thermo-pile, just as it would have been deflected by the battery itself, and this in whatever position the thermo-pile may be. Instead of a hydro-electric current, a magneto-electric or a thermo-electric one may be had recourse to.

On Æthogen and Æthonides. By WILLIAM H. BALMAIN.

AT the commencement of the present year I made some experiments, with the hope of obtaining compounds of boron and silicon with nitrogen; and was successful so far as to obtain compounds consisting of boron and nitrogen together with certain metals, which are possessed of some very remarkable properties. The results of these experiments, with some remarks upon their bearing upon the science of chemistry, and a few facts proving the existence of analogous compounds of silicon and nitrogen, were published in the Phil. Mag. for October, 1842. Since that time I have succeeded in isolating the compound of nitrogen and boron, and have given it the name of *Æthogen*, from *αἶθων* and *γείνομαι*, because it produces, by uniting with the metals, compounds which glow with a peculiarly beautiful phosphorescent light when heated before the blowpipe in the oxidizing flame. And I think its compounds may with propriety be named *Æthonides*.

Preparation of Æthogen.—Heat to redness seven parts of finely powdered anhydrous boracic acid with nine parts of melon, in a crucible lined with charcoal; and immediately the crucible is sufficiently cool to admit of being handled, rapidly transfer the light coherent powder which will be found in the interior, to a perfectly dry well stoppered bottle.

Properties.—A white powder, light as prepared magnesia, infusible, and fixed at a white heat. Heated before the blowpipe it burns rapidly, giving the flame a green colour, but without phosphorescing. Exposed to air, for a few seconds and then heated in a tube, it yields a palpable quantity of ammonia. Heated with hydrate of potass, it yields ammonia abundantly. It is not altered by hydrogen at a low red heat, nor by chlorine at ordinary temperatures, nor by the vapour of iodine; and it is insoluble in water, but communicates to it an alkaline reaction. It is decomposed with effervescence by nitric and sulphuric acids, and there remains after evaporation boracic acid. It deflagrates with chlorate of potass and with nitre. Heated to redness with potassium and zinc it yields *æthonides* of those metals.

Æthonide of Potassium. Preparation.—Take seven parts of finely powdered boracic acid and twenty parts of cyanide of potassium, free from water, and, as far as possible, from cyanate of potass and

iron; and having lined a Hessian crucible with a paste of powdered charcoal and gum, and heated it until all water has passed away, place the mixture in the crucible, cover it by inverting and luting a smaller crucible over it, and heat it to whiteness for an hour: it is advisable to use a crucible as a cover, that there may be sufficient room for spurious sublimation, and the rent hole should be bored in the bottom of this crucible and not in the luting at the side; and further to avoid the penetration of oxygen to the materials, it is well to line the upper in like manner with the lower; or, by heating together potassium and æthogen avoiding an excess of potassium, and heating the result with nitric acid to free it from excess of æthogen.

Properties.—A light white solid, infusible and insoluble even when heated in water, in solution of potass, hydrochloric acid, sulphuric acid (strong and diluted), nitric acid and solution of chlorine; it is not altered upon exposure to air, nor does it affect the most delicate turmeric paper when placed upon it in a moist state. Heated with hydrate of potass or soda, it yields ammonia abundantly. In the deoxidizing flame of the blowpipe it is not altered, nor does it communicate any colour to the flame: but in the oxidizing flame it gives a strong green colour, and gradually fuses, yielding a perfect bead which is transparent, hot and cold; and when placed with a drop of water upon test papers, turned turmeric brown and red litmus blue. When the outside flame impinges upon a large surface of the substance in powder, as when a glass tube soiled with it is held at the extreme point of the flame, it presents a beautiful green phosphorescence, owing no doubt to the formation of boracic acid at the surface, and if it be removed to the inner flame, the centre will incandesce, while the outer edges, where it meets with the oxygen of the air, will still yield the beautiful elegant green. When thrown upon fused chlorate of potass it deflagrates with a soft green light, and it will also deflagrate with nitrate of potass. It is not altered by being gently heated with potassium or sodium, nor when heated before the blowpipe on charcoal with lead, zinc, &c. Chlorine has no action upon it at a low red heat, and iodine, sulphur and corrosive sublimate may be sublimed from it without decomposing it. It is not decomposed by hydrogen at a red heat, but below that temperature is decomposed with the evolution of ammonia by the vapour of water, or by any substance which will yield water, as hydrate of potass, hydrate of lime, common clay, hydrated phosphoric acid and the rhombic phosphate of soda. It is not decomposed by hydrochloric acid at a low red heat, and I think it is not altered by hydrofluoric acid, for a small portion of it was mixed with a large quantity of fluorspar, with more than sufficient sulphuric acid to make it all into hydrofluoric acid, and heated as long as fumes passed off, when, after the sulphate of lime had been washed away with dilute nitric acid, it still yielded ammonia with hydrate of lime.

Æthonide of Zinc. Preparation.—Heat together, to whiteness, in

a lined crucible, one part of anhydrous boracic acid and two and a half parts of cyanide of zinc—or heat finely granulated zinc with æthogen to the temperature at which zinc sublimes, and wash the result with nitric acid.

Properties.—A white solid resembling the last, gives ammonia abundantly when heated with a mixture of hydrate of lime and carbonate of potass, and is insoluble (with or without heat) in water, sulphuric acid, hydrochloric acid, nitric acid, solution of potass, and ammonia. It is not decomposed by chlorine or hydrogen at a *full red heat*, nor by corrosive sublimate, nor by potassium or sodium. Before the blowpipe it is infusible, but in the oxidizing flame communicates a green colour, and when at the outer edge emits a *very brilliant* bluish phosphorescence, which appearance it also produces when simply dropped into the flame of a spirit lamp. Thrown on fused chlorate of potass, it deflagrates with a faint blue light.

Æthonide of lead may be obtained by heating chloride of lead with æthonide of zinc, or by heating boracic acid with cyanide of lead, or by heating together lead and æthogen. It phosphoresces with a green light.

Æthonide of silver may be obtained by heating together chloride of silver and æthonide of zinc, or by heating together æthogen and silver. It is a light white solid, and is not acted upon by any of the re-agents with which it was tried, not even by chlorine or hydrogen at a full red heat. This compound phosphoresces with a peculiarly fine green light.

I believe I have obtained æthonides of several other metals by heating their chlorides with æthonide of zinc, but the quantities operated upon were too small to give certain results.

I am bound to apologize for sending in my results without an analysis, but the means of doing otherwise are not in my power, and I am in hopes that Dr. Kane will oblige us with more valuable data than I could furnish.—*Phil. Mag. and Journal of Science.*

Report of some Experiments with Saline Manures containing Nitrogen, conducted on the Manor Farm, Havering-atte-Bower, Essex, in the occupation of Collinson Hall, Esq. By W. M. F. CHATTERLEY.

INDUCED by the prevailing opinion that upon the quantity of nitrogen contained in some animal and saline manures, depend their fertilizing properties, it was decided to test, by experiment, the relative value of three saline manures containing that element as a constituent, viz. nitrate of potash, nitrate of soda, and sulphate of ammonia, all these salts, from their commercial price, being within the reach of the farmer for agricultural purposes, provided a sufficient amount of profit for the outlay can be shown, and that of course would be preferred by the agriculturist which yields the greatest profit on the prime expenditure.

The quantity of nitrogen per cent. in each of the pure salts is as follows —

	per cent.
Sulphate of Ammonia (crystals)	18·80
Nitrate of Soda	16·55
Nitrate of Potassa	13·96

This calculation leaves out of the question the adventitious water contained in the salts as they are met with in commerce, and which in nitrate of soda is very considerable, sometimes as much as 10 per cent., and besides, nitrate of potassa usually containing from 2 to 12 per cent. of chloride of sodium, would in each case reduce the per centage of nitrogen. The quantity of solid impurity in the sulphate of ammonia used in these experiments was less than one per cent.

The quantity of gluten in the different samples of wheat, which forms a very considerable item in the determination of the relative value of these manures, was not obtained, in consequence of the error of the foreman, who used the reserved samples for seed; but an approximation may be obtained by comparing the weights of equal bulks, as it is constantly found that the heavier the sample, the more water is absorbed by the flour made therefrom, and consequently the more gluten it contains; by referring to the column of the table which shows the weight of a bushel of each sample, it will be seen, that for the quantities used, sulphate of ammonia (Nos. 2 and 3) is superior to the nitrates of soda or potassa (Nos. 4 and 5).

The object of these experiments having been to approximate the original cost per acre of these manures, rather than their weights, is the reason why equal weights of sulphate of ammonia and nitrates were not used: neither is it believed that the result would at all correspond with the real result of the actual experiment were we to deduct one-fifth from the product of No. 3; indeed, the product of an acre would not then greatly exceed for 112lbs. of manure that for 26lbs.

It will be observed that the produce of the three manures does not bear a relative proportion to the quantity of nitrogen contained in each, but this gives us no reason for believing that such proportion would not hold good had the quantity of water contained in each been allowed for at the time of dressing.

The attention of the practical agriculturist will probably be attracted to the last column of this table, in which is exhibited the profit on the expenditure in each case: taking the average value of the bushel at seven shillings, and of a truss of straw at ninepence, and calculating chaff as the same value as straw for fodder. The difference between a quarter of a hundred weight and a hundred weight and a quarter of sulphate of ammonia is very remarkable, and, as may be easily perceived, is due to the difference of the value of the produce, the increase in the former instance being wholly in

corn, while in the latter it is in both straw and corn, and it is probable that there is some quantity of the manure between these two extremes which would give the largest return relative to the outlay. It is clear, however, that if a farmer have but £1 to lay out in manure, the quantity of sulphate of ammonia to be obtained for that sum would give a larger return if spread over four acres of land than if bestowed upon one.

Sulphate of ammonia was also tried as a top-dressing upon a poor pasture, at the rate of one cwt. per acre; the operation was performed in the evening after a shower of rain; it was observed on the following day that the clover and some of the grass were withered, indicating that the moisture which remained on the leaves had caused the salt to adhere, and there form too strong a solution; in about a week after this period the grass assumed a greener colour, especially in one part of the field which had been dressed twice over to observe the effect: the crop of hay was not weighed, in consequence of rain coming on at the time of making, but was laid at half a load more to the acre than the undressed portion, and no doubt was entertained that by applying such a dressing earlier (it was not applied till the 12th of May), and in a less droughty season, more time would have been allowed for subsequent growth, and a much larger crop would have been obtained. The after-feed was decidedly improved, and very much preferred by the cattle.

On the same date a quantity of sulphate of ammonia at the same rate, was dressed over a single land in a field of *tares*: the leaves of the plant withered, as in the instance of the pasture above-mentioned, the subsequent produce however was *one-sixth* more than any other land of the field. The oats sown with the tares grew extraordinarily after the application, and it is believed, that if applied earlier in the season to the tares as to the pasture, and during dry weather followed by less drought, the leaves of the plant would not have withered, and the effect would in both cases have been more marked.

On the 21st of May a similar quantity, viz. one cwt. to the acre, of sulphate of ammonia, was applied to a field of *clover* in dry weather: in this instance the leaves did not wither, but no increased growth appeared to be the consequence; and a similar result was obtained by the application of nitrate of soda.

To a field of podding peas much infested with slug, the following dressing was applied early in the morning, and by ten o'clock an immense number of the dead insect covered the land:—

Sulphate of Ammonia. . . . 7½ cwt. at 17s. 0d. £6 7s. 6d.

Common Salt 2 cwt. at 1 6 0 3 0

Oil Cake, finely powdered, 7 cwt. at 6 6 2 5 6

This was sown broadcast, at the rate of 1¼ cwt. to the acre (value 14s. 6d.), and besides saving the plant by the destruction of the insect, seemed in a short time to have converted this crop, from the worst of two others in adjoining fields, into the best of the three, and little or none of the withering effect of the leaves ensued.

It will be observed from the above experiments, that sulphate of ammonia appears to have acted better upon graminaceous than upon leguminous plants; for though the crops of tares and peas seem both to have received considerable benefit from the application, such benefit was by no means in proportion to the effect produced upon wheat or oats; the oats sown with the tares, as has been before observed, growing surprisingly after the application, and exhibiting a marked distinction in their rate of increase.

Sulphate of ammonia, as well as nitrates of soda and potassa, comes under the class of stimulating manures; that is, such manures which, at the same time that they supply the plant with one or more ingredients necessary to the crop, enable it to obtain a larger supply of its usual nourishment from the soil and the atmosphere: hence the quantity of nitrogen itself in the crop from the land to which a nitrogenous manure has been applied, is greater than the sum of the nitrogen from the unmanured portion and the quantity contained in the manure so added; and hence the result of the application of a given quantity of such manure will vary upon different soils, and even upon the same soil under different conditions, depending upon its capacity to supply the plant with its general nourishment. On account of this stimulant action, care should be taken in the application of these manures, that the quantity used should vary according to the condition of the plant and soil, as too large a quantity on a good plant, with a soil in high condition, would cause the crop to lodge, and a small dressing can always be repeated if found necessary.

From the above experiments and several others, which want of opportunity has prevented from being carried out so fully, but from which, as an eye-witness, a comparative opinion may be formed, I am led to believe that no cheaper top-dressing than sulphate of ammonia can be applied to wheat or oats on this land, which is generally a heavy clay upon a subsoil of London clay, when the plant requires it, either from its being sickly or thin on the ground, in consequence of the land being somewhat out of condition, whether from unusual wet, bad seed-time, uncongenial spring, or any such like cause. I should add that equal benefit appears to have been derived from its use upon a light gravelly soil upon a subsoil of gravel, upon the same as the London clay formation.

With respect to the quantity of the salt to be used, it may be best to refer to the practical result of a large and small dressing, as shown in the table above (Nos. 2 and 3), and the previous remarks thereon as to the relative produce of straw and corn in each case, and to add, that although the experiment has not yet been made, there seems reason to believe that a better effect would result from the application of, say one cwt. per acre, at three different dressings than all at once, that is about 37lbs, when the crop of *wheat* makes its spring growth, or when *oats* are about two inches out of the ground; a similar quantity about a month afterwards; and again at

the time of the formation of the ear. But a practical difficulty occurs in applying so small a quantity as 37lbs. of any material over an acre of ground: the simplest mode of overcoming this, unless we could use a machine capable of adjustment, similar to what is termed a clover drill, perhaps is to mix the salt with some substance which shall not exert any decomposing action upon it, but so increase its bulk as to enable it to be equally spread over the surface; the best substance, as far as my experience goes, is *common salt* (itself a manure very generally useful), twice the weight of the sulphate being added to make up one cwt., a quantity not difficult to broadcast over an acre: or, if preferred, *soot*, which itself contains both sulphate and carbonate of ammonia; or even such a mixture as that before-mentioned used for peas, but then care must be taken to pulverize the oil cake sufficiently. A method which has been found to be good is to mix *soot* and *salt* in about equal parts, some weeks previous to the addition of the sulphate of ammonia; the salt condenses moisture from the atmosphere and fixes the carbonaceous particles, and these tend to hold the carbonic acid and ammoniacal gases derived from the atmosphere and decomposing organic matters in the soil, and render them of easy access to the plants. If a very speedy effect be required, the stimulating properties of this dressing may be increased, by setting free a portion of the ammonia by a subsequent *very slight* dressing of hydrate of lime (for obvious reasons this mixture should not be made before the sowing); but as such a dressing is always attended with loss of ammonia, this should never be resorted to unless it is otherwise unavoidable, and if possible when rain may be expected to fall. And this leads me to remark on the very common practice, in this county particularly, of mixing fermenting farm-yard manure with lime, by which all the ammoniacal salts are decomposed and the ammonia driven off, and a large portion of fertilizing material absolutely lost: it is difficult however to convince the farmer accustomed to this practice, for the immediate benefit which results is considerable; for in the first place the vegetable tissues are broken up, and put in a condition more easily to be converted into carbonic acid, and hence more easily absorbed by plants, and besides its bulk is materially diminished, so that 20 loads of the mixture contains the vegetable matter thus prepared of perhaps 40 of the manure in its ordinary state, and there can be no doubt is more speedily exhausted.

The manures here referred to, and most other saline manures when used as a top dressing, should be applied when the plant is dry after a shower of rain, or during hazy weather, but not when any continued fall of rain is anticipated; in the first instance it is slowly dissolved by dews, &c., and permeates the soil in every part, and in the latter a considerable portion of its effect is lost as the salt is washed out and carried away in solution by the drains. When the soil is too dry it remains inactive. It is perhaps super-erogative in the present day to insist upon thorough drainage for the

effectual action of any manure ; it must be clear that unless the soil has been well disintegrated and a free passage exists among its particles, neither moisture, air, nor manure, can thoroughly penetrate, and this condition is effected by drainage alone.

With regard to the use of sulphate of ammonia with the drill and depositing it with the seed : in some experiments made to determine the propriety of this mode of dressing the land, the result was a thin and bad crop, arising apparently from its having received a check during its early growth, and much of the seed having been killed as soon as germination took place ; this may perhaps be accounted for by referring to the withering effect upon the leaves, observed in some of the other experiments, which may be supposed to be much more powerful upon the tender radical and plumule in the earliest stages of development, while they are provided in the cotyledons of the seed with a mild and bland nourishment suitable for these tender organs, and before they are prepared for procuring or assimilating the stronger aliments fitted only for more mature plants.

Attention has been particularly directed to sulphate of ammonia on account of its low price as compared with other nitrogenous manures, a point upon which the extensive practical application of any manure must chiefly depend. The price paid was seventeen shillings per cwt. : it is prepared at the Gas Works in Brick Lane by a patent process for purifying coal-gas by means of dilute sulphuric acid, and is very free from impurity.

A specimen of manure sold as Daubeny's sulphate of ammonia at 12s. the cwt. did not give any traces of ammonia when mixed with caustic lime, but consists almost entirely of sulphate of lime, and is worth no more to the farmer than gypsum, which may be obtained at 2*l.* a ton.

This manure is said to be prepared according to the directions of Dr. Daubeny of Oxford, by pouring the ammoniacal liquor of the gasworks upon finely powdered gypsum : even if it were so, the percentage of sulphate of ammonia to be thus obtained, cannot make its value as compared with pure sulphate of ammonia as 12 to 17 ; and its name, "Daubeny's Sulphate of Ammonia," unqualified as it is by any explanation of its composition, is liable to lead the agriculturist unable to detect its nature, into serious loss and error.

I may perhaps be permitted to remark, that the nitrogen of coal is the store accumulated by the vegetation of past ages, before man converted it to his use, but now, that this inexhaustible source of a material so necessary to increase the quantity of food to be obtained from the present race of plants is opened, it is proper to examine the most advantageous mode of employing it, that so great a boon be neither neglected nor wasted : it should therefore seem to be the duty of all who have it in their power, to confirm or refute the accuracy of such experiments as the above, and if, as I cannot doubt, they are found to be nearly correct, to promulgate as much as possible the facts, in order that so valuable a material may be speedily appreciated by the British agriculturist.

In conclusion I have to add, that these experiments were not originally commenced with that attention to rigid accuracy which is called for in strictly scientific investigations, for they were in fact intended to serve as illustrations to a course of practical lectures on the application of science to agriculture delivered on the spot, and this may form some excuse for the omission of certain data which could easily have been obtained, but which did not appear necessary until it was decided to calculate and exhibit the results in a tabular form.—*Ibid.*

On the Formation of Fat in the Animal Body. By JUSTUS LIEBIG, Ph.D., M.D., F.R.S., &c.

IN my published work on "Organic Chemistry, in its application to Physiology and Pathology," I have endeavoured to explain the nutrition of the human and animal organism, according to the present state of organic chemistry. I have pointed out the relation between the nitrogenous food and the nitrogenous constituents of animal bodies, and have considered the non-nitrogenous constituents of the food as the means of the formation of the non-nitrogenous constituents of animals.

The circumstance, that the large class of carnivorous animals do not take any sugar, starch, or gum in their food, leads of itself to the opinion that these substances are not required for proper nourishment, namely, for the formation of blood; and as it appears from the analysis of plants containing nitrogen, that they possess a similar composition to the substances of the blood, it follows also that in the bodies of herbivorous and graminivorous animals, the carbon of the sugar, gum and starch cannot be applied to the formation of the blood. The nitrogen of the nitrogenous ingredients of the food is therefore in a state of combination, in which the elements necessary for the production of the albumen are already present both in number and relative proportions; in the food of the graminivorous animals, we know after all of no other compound which can supply nitrogen to starch, sugar, or gum, for the production of albumen.

As sugar, gum and starch, in their normal state, disappear in the vital processes of graminivorous animals, and as they are given out of their bodies as carbonic acid and water, it follows from such a conversion that they serve by means of the respiration for the production of animal heat.

The disappearance of fat in animals in consequence of disease, or of an increased absorption of oxygen, and its being given out in the form of carbonic acid and water, is a proof that that non-nitrogenous body is converted to the same use as sugar, gum or starch in animal bodies, and for want of other non-nitrogenous food is applied to the respiration.

The further consideration, that the flesh of the carnivorous, which

of all animals eat most fat, contains no fat, and is not eatable; that the fat in the bodies of graminivorous animals increases when the process of respiration, and with it the absorption of oxygen diminishes, through a want of exercise or an increase of temperature, leads to the conclusion that the fat has its origin in the non-nitrogenous food, the carbon remaining in the body in the form of fat when there is a deficiency of the necessary quantity of oxygen to convert it into carbonic acid.

Supported by the example of what certainly takes place in the processes of fermentation and putrefaction, in which sugar and starch, by giving out oxygen or carbonic acid, form new combinations, which, like æther and fusil oil, more resemble fat in their properties than any other known compounds, I have endeavoured to trace out the formation of fat, on the supposition that the carbon of non-azotized substances remains in the animal body in the form of fat.

According to my statement, the fat consequently originates from the non-nitrogenous constituents of the food: let us suppose from sugar, then this must have undergone a chemical change in conformity with my proposition.

The formation of wax from honey which contains none, in the body of the bee, of which, from the experiments of M. Grundlach of Cassel, there can be no doubt, appears to remove every objection to the possibility of such an action taking place.

I never had the least idea of defending in my book the opinion, or even of expressing it, that the fat which was taken in the food of animals did not contribute to increase the quantity of fat in their bodies; but I was not aware of any supply of butter in the grass which is daily consumed by cows, or of tallow, of lard, of goose-fat in potatoes, barley and oats; in the analyses of these substances as at present given, they contain only waxy substances, and that in such a small quantity that I consider the formation of fat could not be attributed to it.

These ideas concerning the origin of fat in animal bodies took a new direction from a note which M. Dumas appended in the *Annales de Chimie* (new series, vol. iv. p. 208) to my treatise on the nitrogenous food of the vegetable kingdom; in this note M. Dumas says—"M. Liebig is of opinion that graminivorous animals produce fat out of sugar and starch, while MM. Dumas and Boussingault consider it as a fixed rule, that animals, of whatever kind, produce neither fat nor any other alimentary substance; that they receive from the vegetable kingdom all their aliments, whether it be sugar, starch, or fat.

"Were the proposition of M. Liebig founded upon fact, the general formula of chemical equivalents of both kingdoms, as defined by MM. Dumas and Boussingault, would be false. But the commission on gelatine has dispelled all doubt, that the animals which eat fat are the only ones in which fat is found to accumulate in the tissues."

The origin of fatty compounds in animal bodies has, through this note, become a question of dispute.

As far as regards myself, I have neither time nor inclination to engage in it; the object of my observations was to leave no doubt of the physiological importance of the fat of animal bodies, as regards the process of respiration. In this view MM. Dumas and Boussingault agree with me.

I think it now right to explain the reasons which induced me to consider that little or no increase of fat in animal bodies was to be ascribed to the ingredients of the food containing fat, consumed by graminivorous animals.

The food which, according to the experience of physicians, has a decided influence on the formation of fat in animal bodies, is that which is richest in starch, sugar, and other substances of a similar constitution.

Rice, India corn, beans, peas, linseed, potatoes, beet, are used in husbandry in large quantity with great effect for fattening, that is, for the increase of flesh and fat. In Bavaria beer is used as a stimulating food for the increase of fat.

Whether much or little importance may be ascribed to the universal experience of husbandry, it is certain that animals which are fed upon these different substances, under certain conditions (abundance of food, little exercise, high temperature, &c.) after some time become much fatter than before. This fat proceeds from the food. Rice, beans and peas have been carefully analysed by various chemists. Braconnot found in Carolina rice 0.13 per cent. of oil, in Piedmont rice 0.25 per cent.; Vogel found in rice 1.05 per cent.

According to these analyses, the organism received from 1000 lbs. of Carolina rice 1.3 lb. or 2.5 lbs., or according to Vogel $10\frac{1}{2}$ lbs. of fat.

Peas contain, according to Braconnot, 1.20 of a substance soluble in æther, which he calls leafgreen (chlorophyll). The bean of the *Phaseolus vulgaris*, according to the same chemist, contains 0.70 of fat soluble in æther. Fresenius obtained from peas 2.1 per cent. of a substance soluble in æther, from linseed 1.3 per cent.

For every 1000 lbs. of peas or beans, the organism receives, according to Braconnot 12 lbs., according to Fresenius 21 lbs. of fat, and from as many beans only 7 lbs. of fat.

Beer, as far as I am aware of, contains no fat: Fresenius obtained from the pulp of the beet-root 0.67 per cent. of a substance soluble in æther.

According to further direct examinations made in our laboratory, 1000 parts of dried potatoes gave 3.05 parts of a substance soluble in æther. This substance possessed all the properties of resin or wax; we will, however, assume that potatoes contain $\frac{3}{1000}$ of their weight of fat. Three one-year-old pigs, fattened with 1000 lbs. peas and 6825 lbs. potatoes fresh boiled, which are equal to 1638 lbs. of dried potatoes, increased in weight in thirteen weeks from 80 lbs.

to 90 lbs. each. A fully fattened pig averages in weight from 160 lbs. to 170 lbs., and after killing the fat weighs from 50 lbs. to 55 lbs. The three pigs have consumed 21 lbs. of fat, contained in the 1000 lbs. peas, and 6 lbs. in the 1638 lbs. of potatoes, together therefore 27 lbs. Their bodies, however, contained from 150 lbs. to 165 lbs. of fat. There is an increase of from 123 lbs. to 135 lbs. more fat than the food contained. A pig one year old weighs from 75 lbs. to 80 lbs.: suppose it to contain 18 lbs. of fat, there still remains, leaving entirely out of question the matters soluble in æther contained in the excrements, 69 lbs. to 74 lbs. of fat;* the production of which in the organization cannot be doubted, and whose formation remains to be accounted for.

M. Boussingault's examinations concerning the influence of food on the quantity and composition of the milk of the cow, furnish other more important grounds for the opinion that animals produce fat out of certain food, which is neither fat itself, nor contains fat.

M. Boussingault's experiments correspond with universal experience, and I believe are to be relied upon; it is, therefore, the more inconceivable to me that he has placed himself by the side of those who support the opposite opinion.

A cow was fed at Bechelbrunn during eleven days upon daily rations of 38 kilogrammes of potatoes, and therefore in eleven days upon 418 kil. Also 3·75 kil. chopped straw; in eleven days 41·25 kil. In these eleven days she gave 54·61 litres of milk, which contained 2284 gram. butter. As 418 kil. of fresh potatoes are equal to 96·97 kil. of dry potatoes (potatoes contain, according to M. Boussingault, 76·8 water and 23·2 solid matter, *Annales de Chim. et de Phys.* 1838, p. 408); further, as 1000 gram. potatoes contain only 3·05 gram. of soluble matter, and the straw, according to experiments made here, contains only 0·832 per cent. of a substance soluble in æther (a crystalline wax), the cow had, therefore, in eleven days consumed 291 + 343 gram. = 634 gram. of substance, soluble in æther. There was contained in this milk however 2284 gram. of fat.

In another case, in a trial carried on in winter, the daily rations of the cow was for a long time 15 kil. of potatoes and 7½ kil. of hay. The quantity of milk amounted in six days to 64·92 litres. These

* M. Vogt, a butcher at Giessen, in answer to some questions of mine, gave me the following as the result of his experience, which has been confirmed by other intelligent persons:—A restless pig is not adapted for fattening, and however great the supply of food it will not grow fat. Pigs which are fit for fattening must be of a quiet nature; after eating they must sleep, and after sleeping must be ready to eat again. When a pig is a year old it weighs from 75 lbs. to 80 lbs., and if the fat is intended to be used as lard, it must be fed daily for thirteen weeks with 20 lbs. to 25 lbs. of boiled potatoes, and about a measure of peas (2 liter.); towards the end of the time the food may be somewhat diminished. A pig so fattened weighs from 160 lbs. to 170 lbs., and contains of fat and lard, taken altogether, from 50 lbs. to 55 lbs. A pig of a year old has a lard membrane under which the lard is secreted, but at that age it does not contain lard.

64·92 litres of milk contained 3116 gram, of butter. In six days the cow consumed 90 kil. of fresh potatoes, equal to 19·88 of dried; in the same time 45 kil. of hay were consumed. Suppose that the 19·88 kil. of potatoes supplied to the cow contained 60 gram. of fat, the other 3056 gram. of butter must have originated from the 45 kil. of hay. According to this, hay must contain nearly 7 per cent. of fat. This is easily ascertained by experiment.

From hay of the best quality, in the state in which it is consumed by the cows, 1·56 per cent. of a substance soluble in æther was obtained in the Giessen laboratory. Taking the hay to contain 1·56 per cent. of butter, the 45 kil. of hay could supply the cow with only 691 gram., there remains, therefore, to discover whence the other 2365 gram. of butter originated which M. Boussingault found in the milk.

In a note which M. Dumas has appended to a communication of M. Romanet's, the following remarks are made :—

“ Hay contains, in the state in which it is consumed by the cow, nearly 2 per cent. of fatty matter. We will show (MM. Dumas and Payen) that the ox which is fattened and the milch cow furnish a *smaller* quantity of fatty material than the fodder contains. As regards the milch cow in particular, the butter in the milk corresponds very nearly with the quantity of fatty material contained in its food; at least as far as in that of the food we have yet studied, namely, hay and Indian corn, which last the cow does not usually obtain as food.”

After the foregoing facts, which I could considerably multiply, it will be difficult for MM. Dumas and Payen to prove that the cow, for instance, furnishes from the fatty matter contained in the food only the corresponding quantity of butter. The proof of the supposition, besides, that animals receive the fat in their food in the same state as it is found in their bodies, is impossible. Nothing is easier to decide than the question whether or not the butter which the cow produces, is contained as butter in the hay.

Hay gives after exhaustion by æther a green solution, and on evaporation a green residue, with a strong agreeable smell of hay, which possesses no properties characteristic of fatty substances. This green residue consists of various substances, of which one is of a waxy or resinous nature, known under the name of chlorophyll; another ingredient of the same crystallizes from a concentrated æthereal solution in minute laminæ, and is the crystalline wax which Proust obtained from plums and cherries, from the leaves of cabbages, from a species of *Iris*, and from grasses, and which is probably identical with the wax that Avequin collected in such large quantities from the leaves of the sugar-cane. M. Dumas has analysed this substance, and found it to differ both in composition and properties from any of the known fats; in consequence of which he felt justified in giving the name cerosine to this substance.

M. Fresenius obtained by means of æther from straw, and M.

Jägle, of Strasburg, from the fresh plant, *Fumaria officinalis* (in the Giessen laboratory), by means of alcohol, a crystalline wax, very similar to cerosine. The occurrence of wax in the vegetable kingdom is very extensive, generally accompanied by chlorophyll.

Margaric or stearic acid, the principal ingredient of the fat of animals, is neither found in the seeds of corn, nor in herbs nor in roots which serve as food. It is evident that if the ingredients of the food soluble in æther are convertible into fat, margarine and stearine must be formed out of wax or chlorophyll.

As far as our experience goes, the chlorophyll of the food taken in a green state is given out from the body unchanged; even in man the excrements retain the colour of the green vegetables consumed. It is also considered that the wax does not experience any change in the organism. All doubt may be removed by the simplest experiments: it may be shown that the excrements of the cow contain as much of the substances soluble in æther as has been consumed in the food. The excrements of a cow which was fed upon potatoes and grass were dried and exhausted by æther; a green solution was obtained, somewhat darker in colour than that given by hay, which upon concentration owed its consistency to a white crystalline waxy substance, which was surrounded by a dark green mother liquor. Upon further evaporation, it gave out an unpleasant smell, and left when dried at 100° C., 3·119 per cent. of the weight of the excrements of fat and similar substances.

As M. Boussingault has found that the dried solid excrements (*Annal. de Chim. et de Phys.* t. lxxi. p. 322,) amount to four-tenths of the weight of the dried fodder, it is evident that these excrements contain very nearly the same quantity of fatty substances as the food consumed.

7½ kilogr. of hay contain (at 1·56 per cent.) 116 gram. of fat. The 15 kilogr. of potatoes contain further 10 gram of fat. In the whole, therefore, 126 gram. of fat.

The solid daily excrements weigh 4000 gram.; they contain (at 3·119 per cent.) 124·76 gram. of fat. A cow which produces in six days 3116 gram. of butter, consumes in its food during the same period 756 gram. of substances soluble in æther, and gives off in her excrements 747·56 gram. of substances of the same nature and properties; it must therefore follow, that in the production of 6¼ lbs of butter in the milk, these ingredientr of the food can have no share.

I consider I have now demonstrated that the fat which accumulates in the bodies of animals during the fattening process, and that the butter daily produced in the milk, do not originate from the wax or chlorophyll of the food, but from the other ingredients of it. I think I should be giving myself unnecessary trouble to look after facts to correct M. Dumas's peculiar opinion, because upon further consideration it is of a nature to correct itself.

It is similar to the idea of M. Payen, that the oil of potato spirit

(fusil oil) is ready formed and contained in potatoes. But since it has been found that the last syrup arising from the preparation of beet-root sugar produces in the distillation of brandy a considerable quantity of fusil oil, no one will doubt its formation during the process of fermentation.

The opinion of M. Dumas is a necessary consequence of the exclusive hypothesis, that animals produce in their organism no substances serving as food (note quoted above), but that they receive all sustenance, whether sugar, starch, or fat, from the vegetable kingdom.

I agree perfectly in opinion with M. Dumas in relation to the substances which serve for the formation of blood; but differ from him in considering it as fully proved, as far as observation extends, that wax is formed in the body of the bee, and fat in the stall-fed cow.

In regard to the principle of M. Dumas, that the organism of an animal is not able to produce any substance serving as food, it is equivalent to saying that the organism produces nothing, but only transforms it; that no combination takes place in its body, when the materials are not present by which the metamorphosis originates. Thus the formation of sugar of milk in the bodies of carnivorous animals cannot take place, for dog's milk, according to Simon, contains no sugar of milk. Thus also fat cannot be produced in their organism; because besides fat, they do not consume any non-nitrogenous food. But starch, gum, and sugar contain, even with their large quantity of oxygen, all the ingredients of fatty bodies; and the formation of butter in the body of a cow, and wax in that of a bee, leave hardly any doubt that sugar, starch, gum, or pectine, furnish the carbon for the *formation* of the butter or of the wax.

It is further certain that the brain (Fremy), the nerves, the blood (Lecanu), the fæces, and the yellow of the egg (Chevreul), contain a substance in considerable quantity with a far smaller proportion of oxygen than the known fatty acids, a substance which hitherto has not been found in the food of gramivorous animals. The formation of cholesterine from fat cannot be supposed without a separation of oxygen or of carbonic acid and water; it must be derived from a substance far richer in oxygen in consequence of a process of decomposition or metamorphosis, which, applied to the case of starch or sugar, explains their conversion into fat in the simplest manner.

In the before mentioned note to the observations of M. Romanet, M. Dumas attempts from the facts quoted in the preface to my Pathology, to weaken the conclusion to which I had arrived concerning the formation of fat in the animal body. These facts concern the quantity of fat in a goose fed upon Indian corn (maize), which corn I have alledged not to contain a thousandth part of fat or fatty substance. The experiments of M. Liebig, says M. Donné

in the *Journal des Débats*, are throughout inexact and false, as MM. Dumas and Boussingault have obtained 9 per cent. of a yellow oil from Indian corn, which M. Dumas had the honour to exhibit to the Academy.

It must be evident to every unprejudiced person, that the fact mentioned in the preface has no necessary connexion with the discussion, concerning the production of fat, in the work itself, or in the appendix; it is not employed in the argument. While writing the preface, a friend of mine communicated to me the result of fattening geese with Indian corn. I found in the *Jour. de Chim. Médicale*, i. p. 353, an analysis of maize by Lespes, in which no trace of a fatty substance is mentioned. I further found by an examination by Gorham, in the 'Quarterly Journ. of Science,' xi. p. 205 that maize contained a particular substance which he called zein, which was extracted by alcohol and could not be fat, as on the authority of Gorham, this zein was not miscible with fat oils. Gorham does not mention any fat oil.

Therefore, according to every fact of which I was aware, maize contained neither fat nor any substance similar to fat. I had not myself at that time entered into any examination of it. The results obtained by MM. Dumas and Payen induced me, however, to undertake an examination of Indian corn, which was grown in my garden.

67 grm. of maize were exhausted by æther. The æther left behind, on evaporation on the water bath, 2·849 gr. of a thick yellow oil.

The weight of this oil amounted to 4·25 per cent. of the seed. The difference in this experiment from that of MM. Dumas and Payen is very great; 9 per cent. is so much that this seed might be used with advantage in the manufacture of oil. I consequently altered the mode of examination by a proceeding which insured a perfect extraction. The seeds were treated with dilute sulphuric acid kept at nearly a boiling heat until they had almost disappeared. The residue was washed, dried and exhausted by æther. 77 grm. produced in this manner 3·594 grm. of a substance soluble in æther. Maize grown in the fruitful fields of Giessen, therefore, does not contain more than 4·67 per cent. I found since also an analysis by Bizio (*Brugnatelli Giornale*, t. xv. pp. 127, 180,) which gives for Italian maize 1·475 per cent. of fat oil.

Maize belongs to those seeds which produce a decidedly favourable influence on the formation of fat; some maize contains no fat (Lespes Gorham), some contains above 4 per cent. of oil, and other maize contains 9 per cent. of fatty oil. According to each individual's view, arguments may be drawn from these observations favourable or unfavourable to the formation of fat in the animal body; but as the analysis of the excrements of the geese was not made, they cannot be taken into account.

The fatty oil obtained in the Giessen laboratory from the seeds of

the maize, completely dissolved in an alkaline carbonate and formed a perfect soap; it consisted of a fatty acid, which probably is formed by the influence of the air on the fat contained in the seed on its becoming rancid.

According to the analysis of Dr. Fresenius, this oil consists in 100 parts of—

Carbon.....	79.68
Hydrogen	11.53
Oxygen	8.79

and possesses, therefore, a composition similar to known fats.

I consider it certain, that the fat which animals take in their food contributes to increase the quantity of fat in their bodies. We have of this certain and decided proof, in the pathological treatment of persons who daily take a considerable quantity of cod-liver oil.

I further consider it probable that oily fat may pass into crystallized fatty acids; and Wöhler's observation, that fusil oil from corn contains a considerable quantity of margaric acid, finds a satisfactory solution by the experiments of M. Mulder, which make the conversion of œnanthic acid into margaric acid probable.

In the Giessen laboratory the observation was made some years ago, that the oleic acid, in the state in which it is obtained from stearic acid manufactories, produces upon rapid distillation more than the half of a fluid product which on cooling becomes as hard as tallow, and upon expression produces 35 per cent. of margaric acid.

These experiments, which are well worth a closer investigation, render it not improbable that hard tallow might be formed out of liquid crystallizable oil.

Whether similar processes take place in animals, in relation to the formation of many of their compounds, to those that take place in plants, is hardly to be doubted.

The observation by Wöhler of the giving out of oxygen by the infusoria, which led him to put the question, whether the nourishment of these was not dependent upon a similar decomposing process to that of plants, which might by accurate examination be soon brought to a decision.—*Ibid.*

Researches on the Decomposition and Disintegration of Phosphatic Vesical Calculi; and on the Introduction of Chemical Decomponents into the Living Bladder. By S. ELLIOTT HOSKINS, M.D.

THE object of these researches was the discovery of some chemical agent, more energetic in its action on certain varieties of human calculi, and less irritating when ejected into the bladder, than any of the fluids hitherto employed.

These indications not being fulfilled by dilute acids or other solvents which act by the exertion of single elective affinity, the author investigated the effects of complex affinity in producing decomposition, and consequent disintegration, of vesical calculi.

For this purpose an agent is required, the base of which should unite with the acid of the calculus, whilst the acid of the former should combine and form soluble salts with the basis of the latter. The combined acids would thereby be set free in definite proportions, to be neutralized in their nascent state, and removed out of the sphere of action, before any stimulating effect could be exerted on the animal tissue.

These intentions the author considers as having been fulfilled by the employment of weak solutions of some of the vegetable super-salts of lead; such as the supermalate, saccharate, lactate, &c. The preparation, however, to which he gives the preference, is an acid saccharate, or, as he calls it, a *nitro-saccharate of lead*.

The salt, whichever it may be, must be moistened with a few drops of acetic, or of its own proper acid, previous to solution in water, whereby alone perfect transparency and activity are secured. He furthermore states, that the decomposing liquid should not exceed in strength one grain of the salt to each fluid ounce of water, as the decomposing effect is in an inverse ratio to its strength.

Having by experiments which are fully detailed ascertained the chemical effects of the above class of decomponents on calculous concretions *out* of the body, the author briefly alludes to the case of three patients, in each of whom from four to eight ounces of these solutions had been repeatedly for weeks together, introduced into the bladder, and retained in that organ without inconvenience for the space of from ten to fifty minutes.

It not being the intention of the author to enter into the medical history of these cases, he merely cites the above facts as sufficient to establish the principle originally laid down; namely, chemical decomposition of phosphatic calculi, by means of solutions so mild as to be capable of retention in the living human bladder without irritation or inconvenience.—*Chemist*.

On the Areas of Subsidence in the Pacific, as indicated by the Distribution of Coral Islands. By JAMES D. DANA, Geologist of the United States' Exploring Expedition.

THE theory of Mr. Darwin with regard to the formation of atolls, or annular coral islands, has been fully confirmed by the investigations of the Exploring Expedition; but his regions of subsidence and elevation, and the conclusion that these changes are now in progress, appear to have been deduced without sufficient examination. Observations at a single point of time cannot determine whether such changes are in progress; they can only assure us with regard to the past. A series of examinations for years in succession is necessary to enable us to arrive at the grand deduction that the land in any part of our globe is now undergoing a gradual change of elevation. The views of Mr. Darwin respecting the rise of the

South American coast, as well as that of the Pacific and East Indies, may well be received with some hesitation. According to my own observations, regions in which his theory would require a subsidence, have actually experienced an elevation at some recent period. I might instance several examples of this elevation in various parts of the Pacific. Suffice it to say here, that I found nothing to support the principle laid down by him, that islands with a barrier reef are subsiding, while those with only a fringing reef are rising; indeed facts most stubbornly deny it. Without entering upon the discussion of these facts which, as they will appear in the government publications, I am not at liberty to dwell upon here, I propose to point out what are the regions of subsidence which the coral islands in the Pacific indicate as having been in progress during their formation.

Before proceeding, I may be excused for adding here a few words in explanation of Mr. Darwin's theory with regard to the formation of coral islands. He rejects the unfounded hypothesis that coral islands are built upon the craters of extinct volcanoes, and proposes the following theory in its stead, which is supported by a minute as well as general survey of the facts.

The coral belt or atoll, he supposes to have been originally a barrier reef around a high island, like the reef around many islands in the Pacific. When the reef commenced, it could not have been extended to a lower depth than one hundred or one hundred and twenty feet, for this is the limit of the reef-forming corals. But if the island gradually subsided—so gradually that the corals could by their growth, keep themselves at the surface, the reef might finally attain any thickness, according to the extent of the subsidence. In this manner, subsidence might finally submerge the whole island, and leave nothing but the reef at the surface. Mr. Darwin points to instances in which only the mountain tops now remain above the ocean. Carry the process a little farther, and we have the coral belt surrounding its little sea—the usual condition of the coral island.

This theory, as is seen, supposes extensive subsidence. And so we remark must every theory: for without it, we could only have reefs one hundred and twenty feet in depth, instead of the great thickness they are believed to possess. It is my present object to fix the area of this subsidence, and suggest something with regard to the extent of it in different parts of the ocean.

On examining a map of the Pacific, between the Sandwich Islands and the Society group, we find a large area just north of the equator with scarcely an island. To the south, the islands increase in number, and off Tahiti, to the southward and eastward, they become so numerous, and are so crowded together, as to form a true archipelago. They are all, too, coral islands throughout this interval. This then is a rather remarkable fact in the distribution of these islands. But let us look farther.

If we draw a line running nearly E.S.E. from New Ireland, near

New Guinea, just by Rotumah, Wallis's Island, Samoa or the Navigators, the Society Islands, and thence bending southward a little, to the Gambier group, we shall have all the islands to the north of it, with two or three exceptions, purely coral, while those to the south, are very generally, high basaltic islands. These basaltic islands are bordered by reefs, and these reefs are most extensive about the islands nearest this line. In the Feejees, the northeastern part of the group contains some coral rings, while the southwestern consists of large basaltic islands with barrier reefs.

Again, to the north of this boundary line, the islands *farthest* from it are usually small, in many instances mere points of reef, a fraction of a mile in diameter, while some of the coral islands *near* the same line are thirty or forty miles in length.

Now a growing coral island or atoll will gradually become smaller in diameter as subsidence goes on, and by the same process must finally be reduced to a mere spot of reef, or, if the subsidence is too rapid, that is, more rapid than the growth of the coral, the island will become wholly submerged and leave nothing at the surface.

On these principles I base my conclusions. Along the equator, as explained, there is a large area containing few islands, and these small, while farther south the coral islands are numerous and large. Is this not evidence, that the subsidence was either more rapid or carried on for a longer period in the former region than in the latter, where they are numerous and large?

Near the boundary line pointed out, stand some of these coral rings enclosing mountain tops, as islets,—as at the Gambier group. Does not this indicate that the subsidence was less here than among the islands *purely coral* to the north? and greater than south of the line, where the reefs are more contracted and the high islands larger and more elevated?

Washington Island (coral), in lat. 5° N., is the last spot of land as we recede from our boundary line to the north-northeast. Beyond is a bare sea, to the Sandwich Islands. Is not this an area where the subsidence was too rapid for the corals to keep the islands at the surface?

It appears then that during this era, the Pacific from 30° N. to 30° S.—and perhaps beyond—was one vast region of subsidence: that subsidence took place most rapidly over the bare area between the Sandwich Islands and the equator, and less and less so as we go from this to the south-southwest. At the boundary line pointed out, it was not sufficient to submerge many of the mountain summits, and south of this, the effect was still less.

This area covers at least five thousand miles in longitude and three thousand in latitude. The seas about the northwest coast of New Holland, show by their reefs, a contemporaneous subsidence, and they should probably be included, as well as some parts of the East Indies. Fifteen millions of square miles is not then an over-

estimate of the extent of the region that participated in this subsidence.

The region of greatest subsidence lies nearly in a west-northwest line, for we may trace it along by Washington Island far towards the arctic coast. The whole broad area of subsidence has nearly the same direction; for this is the course of the boundary line we have laid down as separating the high basaltic and the low coral islands. It is highly interesting to observe that the trend of the principal groups of islands in the Pacific, corresponds nearly with this course. The Low or coral Archipelago, the Society Islands, the Navigators, and the Sandwich Islands, lie in the same general direction, nearly west-northwest and east-southeast. It should be remarked that the Sandwich group does not contain merely the seven or eight islands usually so called; eight or ten others stretch off the line to the north; some, small rocky islets, and others, coral, and the whole belong evidently to one series. I will not say that there is a connection between the trend of these groups and the area of subsidence; yet it looks much like it.

A further point may be worthy of consideration. The Sandwich group consists of basaltic islands of various ages. The island at the northwest extremity, Tauai, is evidently more ancient than the others, as its rocks, its gorges and broken mountains, indicate. By the same kind of evidence it is placed beyond doubt, that igneous eruptions on these islands continued to be more and more recent, as we go from the northwest to the southeast: at the present time the great active volcano is at the southeast extremity of Hawaii, the southeast island. The fires have gradually become extinct from the northwestward, and now burn only on the southwest point of the group. At the Navigators, and I believe also at the Society group, the reverse was true; the northwest island was last extinct. Is there any connection between this, and the fact that low islands are numerous north-northwest of the Sandwich Islands and south-southeast of the Society? Does it indicate any thing with regard to the character of the subsidence in these regions?

The time of these changes we cannot definitively ascertain, neither when the subsidence ceased, for it appears to be no longer in progress. The latter part of the tertiary and the succeeding ages may have witnessed it. Although I am by no means confident of any connection, yet for those who would find a balance motion in the changes, I would suggest that the tertiary rocks of the Andes and North America indicate great elevation since their deposition; and possibly during this great Pacific subsidence, America, the other scale of the balance, was in part undergoing as great or greater elevation.

But why if the western American coast was rising, do we find no corals on its tropical shores to indicate it? The cold extra-tropical currents of the ocean furnish us with a satisfactory reply.—*American Journal of Science and Arts.*

Notice of certain Siliceous Tubes (Fulgurites) formed in the Earth: in a Letter from C. E. WEST, dated New York, March 21, 1843.

A LAMBERT flame was seen playing at night upon the surface of a sand bank, some seventy or eighty feet high, which forms the east bank of the ancient channel of what is called Fish Creek. This excited the curiosity of the neighbourhood and led to an examination of the spot. After removing some twelve or eighteen inches of the soil, they discovered an irregular tube of very coarse glass, which had evidently been made from the sand of the bank. The sides of the tube were compressed, and very irregular. Its longest diameter was about half an inch. Its interior was highly glazed, while its exterior was rough, being covered with particles of sand. When they had exposed about fifteen feet of the tube, they found it necessary to sink a shaft of logs to prevent the caving in of the bank. They continued to dig thirty feet deeper, when it was discovered that the tube, which had maintained an almost vertical position, made a sudden inclination and passed deeper into the bank. The fear of inhumation now compelled them to relinquish all further effort in tracing its course. They, however, dug five feet more in a vertical line and came to water; making in all rising of fifty feet from the surface. The tube was single for some distance from the top, where it made two bifurcations. Some eighteen inches below the surface were found thin strata of indurated sand, which were easily broken by the shovel; they were highly inclined, and their surface was undulating. Some of them were separated from each other one or two inches, others three or four inches. These interstices were filled with sand, which, by digging, had shaken out in some instances, and left the strata like the leaves of an open book; they were glazed, but not so highly as was the interior of the tube.

From this narration of facts, two questions naturally suggest themselves. 1st. In what manner was this tube formed? and 2d. What was the source of the light?

Without attempting to offer satisfactory replies to these questions, I would remark, in relation to the first, that sand tubes of a few feet in length have been frequently described, but none of them, so far as I know, equals in interest the one referred to. None had its great length. To account for their formation, several theories have been proposed. One, that carbonate of lime held in solution had been gradually deposited around vegetable stalks, which finally wasted away, leaving these peculiar tubes. Another, that they are the work of insects. The third and most popular theory is, that they are produced by *lightning*.* It has been suggested, that whenever the electric fluid in its passage into the earth meets with the essential ingredients of glass, it fuses them into these singular tubes, provided the current be of sufficient intensity. It appears to me that neither of these causes is adequate to produce a tube *fifty* feet in length, such as we have described. That the first two had any

* Hence the name *Fulgurites*, by which they are usually known.

agency in the matter, we cannot admit for a moment; for the tube gives evidence of igneous action, and consists of silex instead of lime. With respect to the third, let us inquire, if from the diffusive tendency of electricity to divide itself into a thousand ramifications on coming in contact with moist bodies, is it probable that the fluid would pass for fifty feet or more in a continuous line through moist sand? It strikes us as highly improbable. Again, if lightning is the cause, why did it not produce a *solid mass* instead of a tube? And yet, if we set aside these objections, the tube appears as though it were formed in this way. The smooth and highly glazed surface of the interior, admitting atmospheric electricity to be the agent, might be accounted for from the fact of its being nearer the central action of the fluid, and also from the fact that there would be no particles of unmelted sand within the tube to mar its surface, while the exterior in its liquid and afterward pasty state, coming in contact with particles of sand, would be pierced by them and made rough. It would be natural to suppose that a tube produced in this manner would collapse, presenting a flattened appearance.

With regard to the second question, it is now impossible to tell what the gas was which produced the light, because it has disappeared since the destruction of the tube. It may have been phosphuretted hydrogen, derived from the decomposition of animal bones deposited ages ago beneath that sand bank, or it may have been pure hydrogen, resulting from the changes which native protosulphuret of iron undergoes when exposed to moisture; for it is well known in the spontaneous decomposition of water by this mineral when thus exposed, that it absorbs the oxygen of the water, forming a protosulphate of iron, and eliminates heatsufficient to inflame the hydrogen; or it may have been sulphuretted hydrogen derived from the decomposition of iron pyrites, the bisulphuret of iron, which is often associated with organic remains, which would also afford phosphuretted hydrogen, thus yielding a mixture of these gases, one of which burns spontaneously at ordinary temperatures.

The writer has made these gratuitous comments, not with the intention of satisfactorily accounting for these phenomena, but for the sake of awakening enquiry among your readers upon this interesting subject.—*American Journal of Science and Arts.*

Loadstone in the Ashmolean Museum, Oxford.

THIS fine loadstone was presented to the University of Oxford by Maria, Countess of Westmoreland, in the year 1756. It is elegantly mounted in an ornamental brass case, and armed with soft iron at its poles. It hangs freely at a convenient height for the operator to show the whole arrangement, and exhibit the power of its attraction, which at this time is about 160 lbs., having gained 25 lbs. of attractive force since the time it was first deposited here. The weight of the stone, armatures, and case, amounts to about 171 lbs.—*Correspondent.*

The Comet and the Zodiacal Light. By SIR J. S. W. HERSCHEL,
K.H., F.R.S.

THE zodiacal light, as its name imports, invariably appears in the zodiac, or to speak more precisely, in the plane of the sun's equator, which is 7 deg. inclined to the zodiac, and which plane, seen from the sun, intersects the elliptic in longitude 2 sec. 18 deg. and 8 sec. 18 deg. or so much in advance of the equinoctial points. In consequence it is seen to the best advantage at, or a little after, the equinoxes, after sunset at the spring, and before sunrise at the autumnal equinox, not only because the direction of its apparent axis lies at those times more nearly perpendicular to the horizon, but also because at those epochs we are approaching the situation when it is seen most completely in section.

At the vernal equinox the appearance of the zodiacal light is that of a pretty broad pyramidal, or rather lenticular body of light, which begins to be visible as soon as the twilight decays. It is very bright at its lower or broader part near the horizon, and (if there be broken clouds about) often appears like the glow of a distant conflagration, or of the rising moon, only less red; giving rise, in short, to amorphous masses of light, such as have been noticed by one of your correspondents as possibly appertaining to the comet. At higher altitudes its light fades gradually, and is seldom traceable much beyond the Pleiades, which it usually, however, attains and involves; and (what is more to my present purpose) its axis, at the vernal equinox, is always inclined (to the northward of the equator) at an angle of between 60° and 70° to the horizon; and it is most luminous at its base, resting on the horizon, where also it is broadest, occupying in fact an angular breadth of somewhere about 10° or 12° in ordinary clear weather.

When, therefore, a beam of light of a nearly uniform breadth, not exceeding a degree or a degree and a half at the utmost, inclined at an angle of not more than 25° to the horizon, and that not to the north but to the south of the equator, and making an angle of 33° with the zodiac to the southward, instead of 70° to the northward of that circle—when that beam of light, not increasing in intensity towards the horizon, but quite the contrary, is seen in the sky at or near the time of the vernal equinox, I think it will be granted at once that no observer familiar with the appearance of the zodiacal light, and bearing in his recollection its geometrical relations to our globe, could for a moment be deceived into regarding such a beam as any possible phase of the last-mentioned phenomenon.

But to come to particulars. I must now observe, that on Friday the 17th, and on every evening since that time when I have observed the comet, the zodiacal light has also been displayed in the most striking and perfectly characteristic manner, and, indeed, with a brilliancy which I should certainly be disposed to regard as a proof

of the opinion generally entertained of its varying from season to season in intensity, were it not for the habitual and exceeding purity of the atmosphere in this favoured corner of our island, which is altogether free from smoke, and most commonly so from haze, and (in the absence of actual cloud) quite upon a par (so far as a three years' residence will enable me to judge) with that of any region I have visited.

The zodiacal light, then, on every occasion when I have seen the comet, has been thus superbly displayed, occupying its usual place among the stars, and having all its usual characters, while the comet in no part of the extent of its tail so much as touched upon the region occupied by it.

Furthermore, Mr. Cooper distinctly states that he saw the nucleus at Nice, and, as that gentleman has given proofs enough of his acquaintance with the appearance of comets, I cannot suppose him to have mistaken any other object for one.

Lastly, I have myself, on one occasion, distinctly seen the head with its so-called nucleus. In this also I could not be deceived. The telescope with which I viewed it (having no night-glass or comet seeker at hand) was only a 20-inch achromatic of $1\frac{1}{2}$ -inch aperture, with a single lens for an eyepiece, and magnifying power of about 12 or 13, held in the hand. With this I found it, after no very long search, near a star which (misled at the time by the magnitude ascribed to zeta Eridani in Bode's map) I supposed to be that star, but which, on consulting better authority, I now consider to be zeta of that constellation, or possibly eta; and after several times laying aside the telescope and contemplating the tail, as often returned to it, having not the least difficulty in finding it again.

But now comes the most remarkable circumstance attending the appearance, or rather disappearance of this comet. The next night (Saturday) I prepared a seven feet Newtonian reflector, of six inches aperture, on the roof of my house, expecting to obtain a good view of the nucleus. To my amazement, though the night was clear and the horizon good, I could not find it; but I did find, in the very central line of the train, near no star, nor in any identifiable place, a dim, pretty large, oval nebula, very little condensed towards the centre, but with no appearance whatever of a nucleus. This nebula I also several times swept over; so that I have not a suspicion remaining on my mind of the possibility of any illusion. Now, there are nebulae in that region of the sky; but on referring to a catalogue of of them I do not find one which I consider it would have been possible to have seen with such an instrument and under such circumstances as remaining twilight and vicinity to the horizon. Moreover, the next night (Sunday), taking up the observation earlier and pursuing it later, with the same reflector and an equally good sky, this nebula was also missing. These facts I consider as well worthy to be placed on record, and to indicate a rapidity of diminution in point of lustre only to be explained on the supposition, that the

comet is receding from us with great velocity. The train, too, is diminishing rapidly in brightness, though it retains its position with remarkable pertinacity; at least it did so on Wednesday night.

J. S. W. HERSCHEL.

May, 1843.

Means of Communicating to Fecula, without the assistance of Torrefaction or the Acids, the property of dissolving in Water at 160° Fahr., and of retaining this solubility for twelve months or more.
By M. JACQUELAIN.

A SOLUTION of diastase was prepared at 140° F., with 300 grammes of pure water, and 80 grammes of germinated barley. The filtered solution was then divided into two equal parts, one to be used for moistening 125 grammes of fecula dried in the air, the other to be used in a similar way with 125 grammes of fecula previously dried at 212° Fahr., in order to facilitate the imbibition of the liquor by the fecula.

An hour after this moistening, each portion was set to drain on blocks of plaster, then their desiccation was terminated in a platinum capsule maintained at 104° F., by the water of a sand-bath.

It is evident that in thus placing the moistened fecula on plaster, I wished to accelerate the flowing of the liquid in excess, and to prevent the alteration of humid diastase which so promptly takes place in contact with the air. These preparations being terminated, it was necessary, on one hand, to ascertain whether fecula impregnated with diastase had acquired the property of dissolving in water at 160° F.; and, on the other hand, if the fecula could long retain this solubility.

The first question has been affirmatively answered, for 5 grammes of these two feculas mixed with 30 grammes of water, gave a complete and very fluid solution as soon as the water had acquired the temperature of 160° F.

The results were the same when, instead of operating as above, the 5 grammes of each fecula were added to 30 grammes of water at 140° F.

These experiments, tried on the 25th March, 1841, were repeated with equal success at the same period of 1842; and when the fecula thus prepared was employed, which had been designedly kept in vessels simply covered with a paper, the solution in water was always obtained. In 1843, these same feculas no longer dissolved in water at 160° F.

From these facts, it evidently results that diastase, a principle which is eminently ephemeral in the isolated state, may nevertheless be transported into fecula, and be preserved in it for some time from the variations of temperature and from atmospheric humidity, just as it is kept in barley, whose desiccation has been suspended by a suitable process.—*Chemist.*

The Antarctic Expedition.

WE congratulate the country on the happy return of one of the most memorable expeditions ever recorded in English history; an expedition fortunate in every respect, in the outfit provision made for its success, in the intrepidity and skill of its conduct throughout, in the perfect accomplishment of all its scientific objects, and finally, in its auspicious arrival at home, after four years of brave and unwearied exertion, in safety, to be crowned with the rewards and honours so nobly earned from an admiring and grateful nation. Three fine fellows were lost by accident within the four years; but such was the effectual care and management bestowed during all that time, under every circumstance of toil and danger, that the first natural death occurred at Rio, on the homeward voyage, and the first and only corpse was there committed to the earth.

The Erebus, Captain James Ross, and the Terror, Captain Crozier, left England on the 29th of September, 1839, and made observations at Madeira, Port Praya, St. Paul's Rocks, and Trinidad. On the last day of January, 1840, the expedition reached St. Helena, Capt. Ross having been desirous, in taking this course, to determine the important point of minimum magnetic intensity, and the nature of the curve connecting those points in which that intensity is weakest. This he accurately accomplished; and we may note, that the large space of Atlantic Ocean so traversed possesses the least magnetic intensity of any portion of the surface of the globe. Kergulen's Land was reached on the 12th of May. Geological and geographical investigations were carried on here. Large fossil trees were found in the lava, and indicated the igneous origin of these islands. Extensive seams of coal were also embedded in the volcanic mass, which may, with great benefit, be employed for the purposes of steam navigation in this quarter of the world, and be of immense importance to the commerce of India.

FIRST YEAR.—From Hobart Town, Van Diemen's Land, the expedition proceeded to Auckland Islands, and completed a perfect series of magnetic observations on the important term day of November, 1840. On the 12th of December, passed through numerous icebergs to the southward of 63 lat., made the Pack-Edge, and entered the Antarctic Circle on New Year's Day, 1841. This pack was not so formidable as represented by the French and Americans. Thick fogs, however, ensued, and, with light winds, rendered their course more difficult as well as tedious; and constant snow-showers impeded their operations. Whenever a clear glimpse could be obtained, they were nevertheless encouraged by seeing a strong water-sky to the S.E.; and on the morning of the 9th after sailing about 200 miles through the pack, they gained a perfectly clear sea, and bore away S.W. for the magnetic pole! Jan. 11, lat. 70 47 S., and lon. 172 36 E., land was discovered at the distance of nearly 100 miles, directly in their course, and between them and the pole—the

southernmost known land ever discovered, though somewhat nearly approached by the Russians twenty years ago. As those who accomplished this honour for their country approached, it was seen to rise in lofty mountain peaks of from 9,000 to 12,000 feet in height, entirely covered with eternal snow, and the glaciers projecting from the vast mountain brows for many miles into the ocean. By and by exposed patches of rock were visible; but the shores were so lined with bergs and pack-ice, with a heavy swell washing over them, that a landing could not be effected. They therefore steered to the S.E., where were several small islands, and on the 12th Capt. Ross landed, accompanied by Capt. Crozier and a number of officers of each ship, and took possession of the country, in the name of our Gracious Queen Victoria. The island is composed altogether of igneous rocks, and lies in lat. 71 56 S., and lon. 171 7 E. They steered along this magnificent island; and on the 23d Jan., reached 74 15 S., the highest southern latitude that had ever been previously attained! On the 28th a mountain 12,400 feet above the level of the sea was seen emitting flame and smoke in grand profusion, which splendid volcano received the appropriate name of Mount Erebus. Its position is lat. 77 32 S., lon. 167 E. The advanced period of the season in this high latitude now rendered return advisable. The whole southern land thus traced extends from nearly the 70th to the 79th degree of latitude, and was distinguished by the name of our beloved Sovereign.

SECOND YEAR.—The expedition went to Sidney and the Bay of Islands, in order to extend the magnetic observations, and finish meteorological and other philosophical experiments. It was again New Year's Day, 1842, before they could cross the Antarctic Circle. By January 19 they had succeeded in reaching within a few miles of the open water, when a violent gale sprung up and placed them in a situation of appalling jeopardy. The rudder of the *Erebus* was shattered, and that of the *Terror* was soon after utterly destroyed; and violent shocks against the ice for twenty-six hours, as they rolled deeply among its heavy masses, severely tried their strength, and threatened their existence. On the 21st the gale abated; and though driven back far into, and closely beset by, the pack, they went to work to repair damages and prepare for new efforts. Their condition was very helpless, and their vexation the greater, as the last days were fast shortening, and the season drawing to a close. On the 12th March, in a heavy breeze, the ships were driven into violent collision with an extensive chain of icebergs, and the bowsprit, foretopmast, and some smaller spars of the *Erebus* carried away and lost. The vessels were providentially preserved from being dashed to pieces; and the coolness, promptitude, and activity of their crews, were never more energetically displayed. A direct course was held for Cape Horn, as far from the tracks of former navigators as possible; and in a heavy gale, James Angely, quarter master, fell overboard and was drowned, the only casualty during 136 days of

arduous duty, and again without one man on the sick list. Provisions were supplied from Rio de Janeiro, and the ships were put in as complete a condition to renew operations as the day they sailed from England.

THIRD YEAR.—On the morning of December 17, 1842, the expedition sailed from the Falkland Islands, and on the 24th saw the first icebergs, when nearly in the latitude of Clarence Island; and next day their progress was arrested by a rather solid pack. On the 28th they discovered land, extending S. to S.W.; but its shores lined with so extraordinary an accumulation of grounded iceberg, as to prevent all approach nearer than three or four miles. On the 4th January, 1843, in latitude $64\frac{1}{4}$ S., the ships were beset, drifted rapidly back to the northward. Next day they were extricated and finally succeeded in landing on an island at the extreme of a deep inlet on the south side of the gulf, of which Captain Ross took possession in her Majesty's name. This island is of volcanic origin, and though not more than two miles in diameter, projects a perfectly formed crater to the height of 3,500 feet above the level of the sea. It lies in latitude $64\ 12$ S., and longitude $56\ 49$ W. A magnificent table-topped mountain to the westward rises to the height of 700 feet, and the whole western shore of this great gulf consists of mountainous ranges covered with everlasting snow. It was named the Gulf of Erebus and Terror; it is about forty miles between the Capes, and nearly as many miles deep. The struggles with the ice continued to the 1st of February, when it became essential to extricate the ships, and endeavour to penetrate to the south. They could not penetrate beyond lat. $65\ 15$ S., where their position was 100 miles to the southward of Admiral D'Urville's track where he unsuccessfully attempted to follow the route so nobly achieved by our countryman Weddell. On the 22d they crossed the line of the no-variation, in lat. 61 and lon. 24 W. in a dip of $67\ 49$; a fact of much importance to magnetic science, since the observations appear to prove that the supposition of there being two magnetic poles of verticity in the south (as is well known to be the case in the north) is erroneous, *and that there is in reality but one magnetic pole in the southern hemisphere*: We may notice that the whole of this year's observations tend in a remarkable manner to confirm the position assigned to this pole by Captain Ross from his first year's experiments in its close vicinity. On the 23d March, in lat. $68\ 34$, and lon. $12\ 49$ W., he was becalmed, and seized the opportunity to try for soundings, but 4,000 fathoms of line failed to reach the ground. This great depth is against the probability of meeting with land. For some time, however, they persevered in an attempt to get farther to the south, but the ice was too strong for them, and considerable danger was encountered in a tempestuous gale, which lasted, without interruption, during three days. The darkness of the nights and the number of icebergs seemed only to increase the confidence and courage of the men; and the management of the

ships was, throughout, most worthy of admiration. At length the wind veered to the eastward, and with hearts overflowing with gratitude to God for his merciful protection, when human efforts were all but useless and unavailing, our brave fellows were in safety, and steering for the north. It was not, however, till the 12th that they were relieved from the apprehension of being driven against the still threatening pack. At the end of April, the *Erebus* and *Terror* left the Cape of Good Hope, and touched at St. Helena and Ascension, for the purpose of repeating the magnetic observations they had formerly made, and verifying their instruments. In order to render the whole series complete, it was necessary to repair to Rio de Janeiro, which the expedition reached on the 18th of June. After a few days employed in observing, and refitting, they sailed for England, and, touching at one of the western islands, made the land of Scilly on the 27th of August. The passage up Channel was rendered tedious by calms and light winds, so that Captain Ross was unable to land until Monday the 4th of September, when he disembarked at Folkestone, and arrived in town on the afternoon of the same day. Need we add, that his reception at the Admiralty was most cordial and gratifying.

The acquisitions to natural history, geology, geography, but, above all, towards the elucidation of the grand mystery of terrestrial magnetism, raised this voyage to a pre-eminent rank among the greatest achievements of British courage, intelligence, and enterprise.

Captain James Ross, it is currently reported, is not long to enjoy his ease at home, but is shortly to proceed on another government expedition, to attempt a northwest passage, or at least a North Polar survey.—*Abridged from the Lit. Gaz.*

On a Process of making White Lead, which greatly diminishes the unhealthiness of the work. By M. VERSEPUY.

I TAKE the liberty of addressing to the Academy some developments consequent on a communication made on the 22nd of May, 1843, by M. Gannel, concerning a process of manufacturing white lead.

This same process has for five years been the subject of my occupation in my leisure moments, and while about to finish my observations, I read in the journals the announcement of an analogous work on a new means, which should constitute an epoch in the arts. I cannot have any pretension to priority, since a few days' delay has deprived me of that advantage; may I be permitted, in compensation, to deposit with the Academy a summary of the labours which I undertook with a view to the public interest, and thus to add to those already produced, and which I have proved by experience.

The following is the manipulation at which I arrived:—

The lead in lumps, or pieces of pigs, is thrown into a stone cylinder; I employ lava with an equal weight of water. After 12 hours,

the metallic pap is removed, in order to its being introduced into an immovable wooden cask, furnished with two ventilators on its circular part, fitted with eaves to prevent the escape of the liquid. A wooden stirrer, with paddles, was placed in the interior.

A layer of ceruse is formed on the internal surface of the stone cylinder, which prevents it from wearing. This layer of ceruse arises from the deposit which is formed in the intervals of the operations, and serves as a leaven for determining the oxidation of the particles of lead in the ulterior operations.

It is useless to granulate the lead for a regular manufacture.

The water is necessary for detaching the particles of lead as the friction produces them.

The pap should be extracted from the stone cylinder, in order to separate the lead which is not entirely pulverized, and to be able to submit it to violent agitation, so that the surfaces be multiplied *ad infinitum*.

The carbonic acid of the air is alone sufficient for the formation of the carbonate.

I obtained nothing satisfactory from the introduction of carbonic acid by portions, nor even of an entire atmosphere of that acid into the cask, nor by the employment of acetic and nitric acids and their salts.

It is evident that my process is very cheap and easy of execution.

No addition of any body whatever, no chemical agent, complicates the progress of the operation. The mechanical power alone operates the conversion of lead into ceruse.—*Chemist*.

On a New Process of making White Lead, by means of which the Health of the Workmen is not injured. By M. GANNEL.

IN conclusion, says the author, in terminating his Memoir, my process of manufacture consists,—

1st. In granulating the lead :

2nd. In dividing it indefinitely, by rubbing it on itself in a leaden cylinder :

3rd. In facilitating the oxidation of the divided lead by the introduction of atmospheric air into the apparatus :

4th. In immediately converting this oxide of lead into the carbonate, by employing air more charged with carbonic acid :

5th. In hastening the oxidation of the lead, by introducing into the apparatus some nitric acid, or nitrate of lead :

6th. In washing the product obtained by this process :

7th. In hastening its desiccation, by submitting the result to strong pressure :

8th. In dividing into square cakes the pressed paste :

9th. In drying the divided product in a stove with a current of heated air.—*Ibid*.

Fanshawe's Patent Elastic Pavement.

WE are indebted to Messrs. Ancona and Bagster, the well-known architects and surveyors, whose names alone are sufficient guarantee of the respectability of the undertaking which we are about to notice, for the means of giving our readers a brief account of the advantages attending the adoption of this excellent material for the construction of pavements.

It is composed of caoutchouc, combined, by mechanical means, with other suitable materials, by a method for which Her Majesty's Letters Patent have been granted to Mr. H. R. Fanshawe, a gentleman whose inventive genius has frequently brought his name honorably before the public. To carry out this patent a company has been formed. This composition constitutes a firm, substantial, and elastic pavement; we understand that the most severe tests of pressure, and wear and tear, have been applied to it, without the slightest injury, and we have no hesitation in publicly testifying to its great and indisputable superiority.

We gather from the company's prospectus the following statement of the applicability of this pavement :—

Caoutchouc forms the best material that has hitherto been applied to the paving of stables, court-yards, basements, and foot-paths; for preventing the escape of the highly deleterious effuvia from the vaults of churches or other buildings exposed to the action of noxious gases; for preventing the noise which, in large rooms devoted to religious or civil assemblies, is found so serious an inconvenience; for prisons, hospitals, asylums, &c., where the prevention of damp is so essentially necessary for the preservation of health; for sea and wharf walls; for a protective covering to piles, especially where they are likely to be destroyed by the worm; and for rendering roofs, arches, vaults, &c., impervious to moisture.

Rats and other vermin cannot penetrate it; therefore it is well adapted for the floors and walls of granaries and malhouses. For stables it also offers very considerable advantages. Its resistance of humidity, its elasticity and softness, and its non-liability to fracture (like the materials now employed), are valuable qualities, of which every one possessing a horse would do well to avail himself.

If the pavement of a stable be properly inclined, so that the urine may run into a surface-drain, and if a pail or two of water be thrown over it daily, in order to remove any urine that may remain on it, and if the water be quickly removed with a brush or broom into the main surface drain, there will no longer be any necessity for sub-drains; and there will be no injury to the horses' eyes, from the ammoniacal vapour resulting from the spontaneous decomposition of the urine.

It is very well applied to the flooring of lunatic asylums, as the patients very frequently dash their heads against the ground, and this elastic substance will not hurt them.

It has occurred to us that it would form an excellent flooring for laboratories and shops; and that the thin material might advantageously be applied to all the purposes for which floor-cloth is now used. Indeed, we think that the thick pavement would be an excellent substitute for the boards used for the floors of dwellings, &c. Among other benefits arising from this substitution, would be the impossibility of rats and other vermin making their way through it.

We should say, that, from its noiselessness, it would be extremely well applied to the paving of churches, concert rooms, theatres, and the New Houses of Parliament.

It has been laid down in the stables of the Royal Horse Guards Barracks, Knightsbridge, the Royal Artillery Barracks, Woolwich, and we understand that the stables of the Barracks at Winchester are about to be paved with it.—*Ibid.*

Discussion of some Observations by M. Pelouze on the same Bodies considered in the Amorphous and Crystalline States. By M. GAY-LUSSAC.

AT the sitting of the Royal Academy of Sciences on the 9th of January last, M. Pelouze read a "Memoir on Hypochlorous Acid, with some Observations on the same Bodies considered in the Amorphous and Crystalline States." I fully join in the praises which the first part of M. Pelouze's memoir merits: as to the second—that containing some observations on the same bodies considered in the amorphous and crystalline states—I request permission to make it the subject of discussion.

M. Pelouze has ascertained the oxide of mercury, precipitated from a mercurial solution, was violently attacked by chlorine, whilst the oxide obtained by the calcination of the nitrate or by the direct oxidation of mercury in the air, was acted on only after a long time, although it was in a state of extreme pulverisation. He is led to believe that this difference of action is due to two different states of the oxide of mercury. The precipitated, or amorphous oxide, would alone be susceptible of being decomposed by chlorine, at the ordinary or a low temperature, and the crystallised oxide would resist in the same circumstances. And the objection which would consist in the crystallised oxide of mercury, and especially its powder, giving with chlorine a small quantity of hypochlorous acid, does not appear sufficient to M. Pelouze for rejecting the preceding hypothesis; for this oxide might contain the amorphous modification. "I doubt," says he, "that the chemists who may repeat my experiments may attribute the differences which I have observed merely to a purely mechanical cause, as a greater number of reacting surfaces in the amorphous oxide. For my part, I should rather believe that pulverisation changes the nature of these surfaces, or sets at liberty small quantities of amorphous oxide concealed in the interior of the crystals."

M. Pelouze was confirmed in his opinion, by calling to mind that Wöhler had found considerable differences in the points of fusion of the same compounds, according as they were amorphous or crystallised. It appeared to him *a priori*, possible that something similar might be manifested in the terms of the composition of oxide of mercury. Indeed, he found that the amorphous oxide on which chlorine exerts so energetic an action is decomposed by heat much sooner than the crystallised oxide. The difference is such, that if these two oxides be placed in the same alloy-bath, or, more conveniently, in the muffle of a cupel furnace, at a properly maintained temperature, the amorphous oxide completely disappears, while the other has scarcely begun to be decomposed.

"Thus, it is certain," continues M. Pelouze, "that the same compound, solely because it is amorphous or crystallised, ceases to manifest the same reactions on other bodies, and that it likewise presents great differences in the term of its decomposition by heat. That is the most active, if I may so express myself, which is soonest decomposed, and that is not peculiar to oxide of mercury. I have recognised the same property in the amorphous and crystallised bin-oxide of manganese, and in Iceland spar. The amorphous compound is decomposed before the same compound in crystals.

"I hope that the experiments which I have detailed in this memoir are of a nature to attract the attention of chemists. They prove how important it is to establish a distinction, even in a purely chemical point of view, between bodies which differ only by a peculiar state of aggregation. It was known, indeed, that certain insoluble bodies obtained by precipitation were more adapted for certain reactions than the same bodies prepared by the dry way: but the differences of cohesion alone were seen, and they were not considered also to extensively modify the chemical properties."

The importance of the question raised by M. Pelouze has determined me to quote his own expressions nearly verbatim: I should have been fearful, in mutilating them too much, of altering the sense of his ideas. Now, it would be more easy for me to discuss them. I wished at first to verify M. Pelouze's experiments on the decomposition of amorphous and crystallised oxide of mercury by heat; for the disappearance of the amorphous oxide in the muffle of a cupel furnace, more speedily than that of the crystallised oxide, did not appear to me conclusive.

I therefore submitted to the action of heat in the same fusible alloy-bath, two glass tubes, of equal diameter, containing equal weights, the one of well crystallised oxide of mercury, made directly by the air, and the other of oxide precipitated from the chloride and well dried. The gases were conducted in small eprouvettes on to a water-bath. The temperature was slowly raised, and, nevertheless, at the same moment, or very nearly so, the metallic mercury exhibited itself in very fine globules on the upper side of the tubes, and oxygen began to be disengaged. Every time that the fire was

lowered, then revived, the disengagement of oxygen ceased, or was resumed simultaneously in both tubes, and these alterations were very frequently repeated, with the same result. Only the decomposition proceeded rather more rapidly with the amorphous than with the crystallised oxide. But this circumstance is easily explained; and it is sufficient, besides, that it be proved that, at the same temperature, the decomposition of both oxides is simultaneously manifested, stopped and resumed.

I have also submitted to the action of heat crystallised marble and carbonate of lime obtained by dissolving some of the same marble in hydrochloric acid, and afterwards precipitating by carbonate of soda. The two carbonates were placed in porcelain tubes, one touching the other, on the same furnace, the heat of which might be regulated at will. A disengagement of carbonic acid began to be manifested at the same time in both tubes, when the temperature was a dull-red heat; but was scanty and very soon stopped. I attributed it to the action of the surface of the tubes on the carbonate of lime. At a higher temperature, towards cherry-red, the disengagement of carbonic acid was again manifested, and at the same time in both tubes. When the temperature was lowered by diminishing the current of air, the disengagement of carbonic acid almost immediately relaxed, then entirely stopped; a rapid absorption took place, and the water of the tub in which the gases were received would have rushed into the porcelain tubes, if safety tubes had not permitted the ingress of air. When the combustion was re-suscitated by re-establishing the current of air, the disengagement of carbonic acid without delay resumed its course, and always simultaneously in both tubes. These variations of temperatures were produced several times, and were uniformly attended with the same results. I could not then explain to myself how it was that M. Pelouze has always seen the amorphous compound decomposed before the same compound in crystals, if our process were the same.* I will observe, for those who may repeat these experiments, that it is not the volume or the weight of the products which should be regarded, since *they* may depend, *ceteris paribus*, on the surfaces exposed to the action of heat, but indeed their simultaneous appearance, diminution, or cessation. However, having operated on equal weight of each body, and having reduced the crystallised body to very fine powder,† I have not obtained great differences in the pro-

* I have since learned, from M. Pelouze himself, that he had appreciated the difference by heat of the oxide of mercury, of carbonate of lime, and of oxide of manganese, in the amorphous and crystallised states, not by the temperature at which the same body might have been decomposed in each of its states, but, indeed, by the quantities decomposed, in the same time, at the same temperature.

† The pulverisation of the crystallised oxide is permitted; for M. Pelouze, in reply to a question asked by M. Thenard at the sitting of the 9th of January, as to whether he had operated on Iceland spar in crystals, stated that he took care to employ it in very fine powder, the same as crystallised oxide of mercury.

gress of the products, either as regards the oxide of mercury or the carbonate of lime.

It is assuredly the inaction of chlorine on crystallised oxide of mercury, and its energetic action on the precipitated oxide, which made M. Pelouze admit, in this combination, two modifications, which might have escaped until now, although sufficiently powerful to modify the affinities. I hasten to say that there exist numerous ones, which cannot be denied; but the more mysterious they are to us, as regards both their cause and their influence, the more circumspection ought we to use in making them intervene in chemical phenomena. For my own part, the difference of the action of chlorine on the two oxides is not anomalous, and I will explain how I conceive it.

Imprimis, and M. Pelouze has himself recognised it, the action of chlorine on the amorphous and crystallised oxides is absolutely the same in water, which is and ought to be rather longer for the amorphous than for the crystallised oxide. Now, what part does this liquid perform in the experiment? I see in it nothing but a solvent; it removes the chloride of mercury as it is formed on the surface of the oxide, and keeps it always *clean*, always the same. It is the most favourable circumstance for the exercise of the chemical action between two bodies. By suppressing the intervention of water, the mode of action of chlorine on crystallised oxide of mercury is still the same; for chloride of mercury and chlorous acid are produced, although in very small quantity. M. Pelouze admits this; but he explains the result by saying that the crystallised oxide may contain the amorphous modification; and as regards the same oxide powdered, which produces rather more chlorous acid, that he believed that pulverisation changes the nature of the surfaces, or liberates small quantities of amorphous oxide concealed in the interior of the crystals. But this is a new supposition, grafted on a former supposition, and it will not be granted that one can give consistence to the other. It remains constant, on the contrary, that the action of chlorine on the two oxides of mercury is the same, since its products are identical, and that there is no other difference between them except the time they require to be completed. The apparent inertness of chlorine on crystallised oxide of mercury, may be perfectly compared to that of rather concentrated nitric acid on carbonate of baryta; if it be diluted with much water, and if the nitrate of baryta formed at the surface of the carbonate be dissolved, the action then becomes very vivid, as between chlorine and crystallised oxide of mercury in the presence of water. It is thus that potassium and sodium decompose water so rapidly only because their oxides are very soluble in it; that many metals, alone capable of decomposing water, but producing insoluble oxides, are but slowly oxidised; whilst, with the assistance of acids which dissolve their oxides, the decomposition of water is effected with great rapidity;* that eminently oxidisable

* It is worthy of remark that no metal which does not decompose water *alone*, decomposes it in the presence of the acids.

bodies remain in presence of eminently oxidising agents without undergoing any perceptible alteration; that the combustion of carbon, the product of which is volatile, for that reason becomes so easy, while that of boron, a far more highly combustible body, is very difficult, and stops as soon as the surface of the boron becomes covered with the finest possible layer of boracic acid, &c.

But it will be asked, why is the precipitated or amorphous oxide of mercury so easily attacked by chlorine, without the intervention of water? Why does not the chloride of mercury which must equally be formed on its surface, prevent the chemical action from being propagated? I think the reply easy.

I will first call to mind, according to the observation of M. Pelouze, that the amorphous oxide, precipitated from a mercurial solution and dried at the ordinary temperature, gives, with chlorine, no other gaseous product than chlorous acid, when it is done in such a way that no elevation of temperature can take place. If the oxide be thrown into dry chlorine at a common temperature, a vivid disengagement of heat and light is manifested, and there is no other gaseous product than oxygen. By cooling the chlorine in a frigorific mixture, the oxygen is replaced by chlorous acid: the same result is obtained by putting in contact with chlorine amorphous oxide previously heated to a temperature of 540° to 710° Fahr.; the oxide, contracted by heat, is more slowly attacked by chlorine, and the temperature is so much the less raised.

By operating at a common temperature with amorphous oxide which has not been calcined, it is easy to obtain constantly chlorous acid without oxygen, by mixing it, before introducing it into chlorine, with a certain quantity of sand or sulphate of potassa reduced to powder. The heat disengaged is moderated by this mixture, and the action between the two bodies is propagated so much the more slowly as the proportion of inert matter is more considerable. With that of about one-third of the volume of oxide which I employed, in my few experiments on gaseous chlorous acid, I constantly obtained a gas very easily absorbable by water without any perceptible residue, although the mixture was not very intimate.

Thus the products of the action of chlorine on the amorphous oxide of mercury, depend on the temperature at which they are formed; but this action is finally accomplished; it may be moderated, but not entirely suspended. It is because the precipitated oxide is in a state of division which may be called molecular, and because the chlorine finishes by penetrating into its whole mass.

It is not the same with crystallised oxide of mercury. It is in small compact masses, which mechanical division cannot reduce to the same degree of tenuity as the precipitated oxide. These combined circumstances easily explain why the action stops almost as soon as it has commenced. The chloride of mercury remains fixed on the surface of the oxide, and however fine the layer may be, it is sufficient to prevent all contact between the oxide and the chlorine,

and to totally suspend their action. Chlorous acid is produced, and proportionally to the quantity and division of the oxide, although always in small proportion; but that is sufficient to demonstrate that the action of chlorine on the crystallised oxide is absolutely of the same nature as that which it exerts on the amorphous oxide, and that there is no difference except in the proportion of the products. If the chloride of mercury were dissolved by means of water, or volatilised or only separated by heat, the results would immediately be the same.

There can therefore be seen in the differences of the action of chlorine on the two oxides of mercury, only the result of a purely mechanical cause, and not that of undetermined modifications. I think that the affinities are more energetic, more profound, and could not be the sport of properties so hypothetical, so undefinable. One of the most remarkable modifications which can be mentioned is undoubtedly that which the carbon in the form of the diamond presents to us; it is evident and palpable, and it would be difficult for the same nature of body to find more different physical properties. And still the proper affinities of the chemical substance of carbon and the diamond remain the same in this disorder of modifications. MM. Dumas and Stas found the diamond to burn in oxygen more easily than anthracite, and as well as ordinary carbon; and it has long been known that there is not an experiment with carbon which does not generally give the same result as with the diamond.

In speaking of modifications, two things have to be considered,—the existence of the modification and its importance in chemical phenomena. Now, if we may judge of this last by the example of the diamond, and by others which might be mentioned, we shall be convinced that this importance is not so great as might at first be supposed, and that physical properties always remain effects of chemical, and cannot profoundly modify them.

In indulging in these observations, it is not with the view of diverting attention from modifications which may be manifested in bodies, but only to guard against the existence which one is sometimes too easily disposed to accord to them; and against the false applications which are made of them, or the importance which is attributed to them. I will mention an example:—

Sulphur exposed to an increasing heat, presents various appearances which are not ordinary, and which modifications have qualified. Frankenheim distinguished three of them, which he designated by the symbols, $S\alpha$, $S\beta$, $S\gamma$.

The first modification $S\alpha$, is sulphur at the moment of fusion, which according to Frankenheim, takes place at 240° F.; it is then transparent and almost colourless. It is this modification which gives the crystallisation in octohedrons, when sulphur is separated from a solvent.

The second modification, $S\beta$, is formed above the fusion of sulphur, and lasts to the temperature of 482° to 500° F.; the sulphur

is no longer translucid, and its colour has become a deep brown. To this modification belongs the prismatic crystallisation of sulphur obtained by fusion.

Finally the third modification commences at the temperature of 482° to 500° F.; it is then brown and almost solid. Heated beyond this temperature, it again becomes fluid, appears almost black, and boils at 782° F.; we then have fused S_{γ} , almost as liquid as water. Between the temperatures of 482° and 500° F., the thermometer remains for some time stationary, in either its ascent or descent.

As sulphur may be evaporated and sublimed below 482° F., it is considered evident that the three isomeric modifications of sulphur have each their gas, and that the deep yellow gas, which weighs three times more than the gas of sulphur should weigh according to calculation, is S_{γ} , and not S_{α} , which, probably, is the modification of sulphur which enters into our ordinary sulphurets.

Berzelius, after having given an account of the ideas of Frankenheim, adds:—"We are now, therefore, in a situation to understand the error committed by calculating the atomic weight of sulphur according to the weight of S_{γ} which experiment gives, and that the true weight of the gas of the sulphur, calculated by means of that of sulphuretted hydrogen, and sulphurous acid, alone is accurate; it is also that which accords with the atomic weight of sulphur. We find, besides, that this difference between the two iron pyrites, which so much embarrassed the crystallographer, Haüy, must arise from one of them containing S_{α} , and the other S_{β} , although we cannot say which of the two modifications belongs to one or other of these pyrites."

I know not whether we are convinced of the existence of the three modifications of sulphur, and of the part attributed to them; for my part, I find that modifications based on shades of colour, on a little more or a little less transparency or consistence, are very vaguely defined, and that their supposed connection with the changes of density in the vapour of sulphur, as well as their existence in the isomeric pyrites, rest on very shallow data. These modifications, and the consequences which may be drawn from them, although supported by the assent of the greatest master of the science, can be considered only as hypothetical conjectures, touching too near the occult sciences. They would, besides, be of very sterile application, since they would leave unexplained the same anomalies, with regard to the density of the vapors of arsenic and of phosphorous, as that which sulphur presents.

M. Pelouze has given, in his memoir, a table of the solubility of chlorine in water at different temperatures. He has remarked that solubility attained its maximum between 9 and 10 degrees, and that it was also at this same temperature that the hydrate of chlorine was destroyed. Taking the volume of water as unity, M. Pelouze found that it took

	vol.	vol.
at 0°	1·75	to 1·80
9	2·70	.. 2·75
10	2·70	.. 2·75
12	2·50	.. 2·60
14	2·45	.. 2·50
30	2·00	.. 2·10
40	1·55	.. 1·61
50	1·15	.. 1·20
70	0·60	.. 0·65

I am occupied with precisely the same subject in my memoir on Cohesion; but proposing to treat in particular of the solubility of some gases, I have quoted for that of chlorine only the maximum which it presents at the temperature of about 468 F. The following are the results which I find in my notes. One volume of water takes at

	vol.	
0°	1·43	of chlorine supposed to
3	1·52	be measured at 32°
6·5	2·08	F., and 0 ^m . 76.
7	2·17	
8·6	3·04	
10	3·00	
17	2·37	
35	1·61	
50	1·19	
70	0·71	
100	0·15	

I will return to this solubility of chlorine, which is only the apparent, and not the real solubility. The latter is obtained by multiplying the first by $\frac{P}{f}$; P being the atmospheric pressure, and f the elastic force of the aqueous vapor corresponding to each temperature. Thus the apparent solubility of chlorine at 70 degrees being 0^{vol}.71, the true solubility would be

$$0^{\text{vol}}.71 \times \frac{0^{\text{m}}.7600}{0^{\text{m}}.2291} = 2^{\text{vol}}.355.$$

This observation applies to the solubility of all the elastic fluids.

M. Dumas added, on this subject, some details concerning experiments which M. Jacquelin proposes to address to the Academy. He has seen, in fact, conformably to M. Gay-Lussac's opinion, that red oxide of mercury, crystallised and powdered with care, acts, with regard to chlorine, exactly like the oxide of mercury obtained by precipitation.—*Chemist*.

On the Composition of an Acid Oxide of Iron (Ferric Acid).

By J. DENHAM SMITH, Esq.

IN the autumn of the year before last, whilst pursuing some investigations respecting the alleged conversion of carbon into silicon, I remarked that when the residuum of the calcination in close vessels of ferrocyanide of potassium (carburet of iron) was fused with carbonate and nitrate of potash, and the resulting compound treated with water, a solution of a deep amethystine red colour was produced; this rapidly decomposed, evolving oxygen gas, and depositing sesquioxide of iron, until the decomposition being completely effected, it became quite colourless; no manganese could be detected in the solution, nor in the deposit, although the colour of the former was precisely similar to the permanganate of potash in solution.

Remembering that the combination of an oxide of iron with potash was already on record, but unable to recall to memory the publication in which it was noticed, I am indebted to the Editors of the *Philosophical Magazine* for referring me to the *Journal de Pharmacie*, tom. xxvii. p. 97, where I found a communication from M. C. le Fremy, upon "The Action of the Alkaline Peroxides on Metallic Oxides."

Since the appearance of this memoir, which has been followed by researches by the same author on the combinations of potash with the oxides of zinc, tin, &c., various notices have been published by MM. Trommsdorff, Wackenroder and Poggendorff, on the subject of this combination of oxygen, potassium, and iron, confirming the existence of this purple-red compound, and pointing out various modes of obtaining it.

I had expected that M. Fremy would have extended his inquiries and ascertained the composition of this new oxide of iron; but as a considerable period has elapsed since his original notice of its existence, and his subsequent researches in connexion with this subject having taken other directions, other chemists also having investigated the compound, and as my own experiments were commenced before I was referred to M. Fremy's paper, I may be excused for having thus directed my attention to the composition of a substance, to the priority of the discovery of which I have no claim, although I had ascertained its existence independently of the observations of the French chemist. I have entered on this explanation, as I would in nowise wish to deprive M. Fremy of his just claim to the original notice of this compound; but as my previous observations and pursuit of the subject were entirely independent of M. Fremy's notice of its existence, I feel myself justified in communicating the results of my investigations, especially as its first discoverer appears to have abandoned the pursuit.

In the memoir in the *Jour. de Pharm.* already alluded to, various modes are pointed out for preparing this combination of potash and oxide of iron, which is obtained by igniting a mixture of sesquioxide of iron, potash, and nitre, or peroxide of potassium and sesquioxide of iron, or by calcining at a full red heat potash and sesquioxide of iron; by these means a brown substance was procured which afforded

a deep violet-red coloured solution, very soluble in water, but which solution is easily decomposed; concentrated solutions of the alkalies precipitate it of a brown colour, which precipitate redissolves on the addition of water; it decomposes rapidly by an elevation of temperature, and instantaneously by the contact of organic substances. The same coloured solution may be formed by passing a current of chlorine through a concentrated solution of potash, holding precipitated sesquioxide of iron in suspension. The German chemists prefer deflagrating dry nitrate of potash with half its weight of iron filings at a temperature approaching to visible redness, and also direct the employment of chlorine gas, potash, and precipitated oxide of iron, as mentioned by M. Fremy, to procure it. M. Poggendorff found that when a current from Grove's battery is passed through a solution of one part of hydrate of potash in four of water, using *cast iron* for the positive pole plunged in the potash, and wrought iron or any other convenient metal as the negative pole, ferrate of potash was formed in the solution, which immediately becomes dark red and opaque. Wrought iron and steel do not produce this compound, but evolve oxygen gas; the red solution soon decomposes either with or without the continuance of the passage of the electrical current, oxygen gas being liberated and sesquioxide of iron precipitated.

To prepare this compound I have found the subjoined process attended with very uniform success:—Wash the ferri sesquioxylum of the shops with boiling water until free from sulphate of soda, dry and ignite at a low red heat; this furnishes a very pure oxide of iron and in a state of minute division; one part of this is to be intimately mixed with four of dried nitre, reduced to fine powder; place this mixture in a crucible of about twice the capacity of the bulk of the mixture, lute a well-fitting cover on, making a few small holes in the lute to allow the escape of gas, and ignite at a *full red* heat for about an hour, if six or eight ounces are made: the time of ignition depends much on the quantity prepared, and the temperature should never be raised above a full red. When well prepared it presents the appearance of a dark reddish-brown porous mass, rapidly deliquescent on exposure to the air, so that I have found it advantageous to powder it while still warm, when it may be preserved for use in a well-stoppered bottle, apparently for any length of time.

To examine the solution of this substance it is most convenient to employ ice-cold water, as much heat is evolved when it is thrown into water, and the decomposition of the solution is augmented or retarded by the elevation or depression of temperature. On the addition of water it evolves much oxygen gas with effervescence, probably owing to the decomposition of peroxide of potassium; if the water employed be ice-cold and the vessel containing it plunged in ice, the decomposition of the solution will be retarded. By allowing the mixture to subside for a few minutes, a solution is obtained almost free from oxide of iron in suspension, of so deep an amethystine red colour as to be apparently opaque except at the edges, but by dilution its colour is readily perceived; it gradually

decomposes, evolving oxygen gas and depositing sesquioxide of iron; heat facilitates this decomposition; at 212° F. it is completely effected and the solution remains colourless. It is decomposed, evolving oxygen, by sulphuric and nitric acids, chlorine is liberated by the addition of hydrochloric acid; by oxalic acid, carbonic acid gas mixed with oxygen is given off. It affords no precipitate with the salts of lime, magnesia, or strontian, but on the addition of a barytic salt a voluminous crimson-red precipitate falls, which may be washed, collected, and dried, at 212° without changing colour. With the metallic salts, the bases of which are capable of combining with more than one equivalent of oxygen, as nickel, manganese, &c., it produces the superoxide of these metals, but with the salts of zinc and metals combining with but one equivalent of oxygen, it precipitates their oxides and evolves oxygen gas.

When the deep pink-coloured solution is prepared by passing a current of chlorine gas through concentrated solution of potash holding oxide of iron in suspension, or through the deep amethystine solution already described, keeping the vessel cool during the passage of the gas, the solution obtained is of a lighter colour than the amethystine liquid and very permanent, it having been kept for months in close vessels without decomposition being completely effected, the gradual progress of which however is shown by the deposition of a light brown substance (sesquioxide of iron?) This chlorinated solution may even be evaporated and a pink salt obtained, but I have been unable to isolate by any means I have attempted the potash salt of this oxide of iron. I may add, that the characters assigned to these two solutions by the French and German chemists are generally in accordance with my own observations.

Although foiled in my endeavours to obtain the potash salt free from admixture of other salts, I was yet enabled to deduce from it the composition of this new oxide of iron, which in accordance with my suggestion, Sept. 1841, is now usually called ferric acid, and subsequently to confirm this constitution by the analysis of the barytic salt.

The mode of analysis was extremely simple, being founded on the perfect decomposition of the deep amethystine solution at 212° . A solution of the reddish-brown fused mass was prepared with ice-cold water and with the precautions before described; when the insoluble portion had subsided, which it did rapidly, and a portion of the solution had been tested by withdrawing it by a tube and mixing with a large quantity of distilled or of lime water (which latter seems to possess the property of checking the rapidity of the decomposition of this potash salt when very dilute), to ascertain whether the subsidence of the uncombined oxide of iron was so complete that an apparently inappreciable quantity was held in suspension; a certain quantity of this clear amethystine solution was transferred to a retort of a known capacity—leaving, of course, a portion of atmospheric air in the upper part of the body and in the neck of the retort; this quantity of air was accurately determined by previous measurements and graduation of the retort; heat was

then applied, and the contents of the retort raised to violent ebullition which was continued so long as any gas was evolved; these gaseous products were collected over water, and when no more gas was liberated absorption was allowed to take place by withdrawing the lamp from the retort; any air which the retort then contained was added to that already collected, and when the gas had acquired the temperature of the surrounding atmosphere, it was measured, the known bulk of atmospheric air originally contained in the neck and upper part of the retort subtracted from it, and the residue, oxygen, corrected for temperature and pressure.

The oxide of iron deposited during the boiling was dissolved in hydrochloric acid and estimated in the usual way. I subjoin the detail of an analysis as an example:—A retort filled to a certain limit with the solution left 13·75 cub. in. of atmospheric air in the body and neck of the retort, and gave 26 cub. in. of gas at a temperature of 58°, and barometer at 29·9 26—13·75 air=12·25 oxygen corrected, weighs 4·2 grs. The oxide of iron freed from silica by hydrochloric acid and ammonia, gave 16·4 grs. of sesquioxide = 11·48 iron, and 4·92 oxygen + 4·2=9·12 oxygen combined with 11·48 iron. The results of several successive experiments are contained in the following table:—

Exp.	Cub. in. at 60° and 30	Grs. of oxygen.	Grs. ox. iron.	Total wt. of oxygen.	Weight of iron.
1	12·3	4·2	16·4	9·12	11·48
2	9·88	3·37	12·6	7·15	8·82
3	13·03	4·45	16·8	9·49	11·76
4	5·21	1·78	7·2	3·94	5·04
5	14·70	5·03	22·3	11·72	15·61
6	7·58	2·59	10·6	5·77	7·42
7	33·41	11·41	42·0	24·01	29·40
	96·11	32·83	127·9	71·20	89·53

The mean of these experiments gives 22·27 parts of oxygen by weights to 28 parts or one equivalent of iron, which is equal to a deficiency of 1·74 part in three equivalents, or 24 parts of oxygen. This is, I submit, an approximation sufficiently near to three equivalents of oxygen to one of iron, to justify me in adopting the formula FeO^3 , as representing the constitution of this acid oxide of iron; especially when we consider that the circumstances under which the experiments were made would indicate the probability of some free sesquioxide of iron being suspended in the solution, as, from the unstable nature of the compound, a short period of time only could be allowed for the deposition of any insoluble matter, and the impossibility of subjecting it to filtration, owing to its decomposition by the contact of organic substances. This excess of oxide of iron varies in the different experiments quoted, so that whilst Nos. 4, 5 and 6 afford 21 to 22 parts oxygen to an equivalent of

iron, 1, 2, 3 and 7 all exceed 22 of the gas to an equivalent of the metal, the last, No. 7, especially; and this experiment being the one upon which I am inclined to place the greatest reliance from the concentrated and clear state of the solution, gives nearly 23 parts (22·85) of oxygen to 28 iron. This cause, together with a certain amount of decomposition during the transference from the vessel in which the solution was made to the retort, affords an explanation of the deficiency in the quantity of oxygen as indicated in every experiment, in comparison with that which I theoretically assign to this compound.

In no one case was the quantity of oxygen actually obtained so small as to indicate a compound of 20 parts of oxygen to 28 of iron ($2 \text{ Fe} \times 50$ or $\text{Fe}^2 \times \text{O}^2$), and I do not consider the formula intermediate with this last and the constitution I have assigned to the oxide, is a very probable one to exist, being $3 \text{ Fe} \times 0 \text{ O}$; moreover, in four out of seven experiments more oxygen was obtained than such a compound could evolve.

The conclusion at which I arrived, that this ferric acid is a *ter-oxide of iron*, is confirmed by the subsequent examination of the insoluble crimson compound it forms with barytes. This salt is readily formed by adding an excess of a dilute solution of a barytic salt to the clear amethystine solution, which, prepared as I have described and dissolved in close vessels, will not contain the sulphates in any marked quantity, nor any carbonic acid whatever, the precipitate being washed in vessels excluded from the atmosphere with freshly boiled distilled water, to prevent the access of carbonic acid to the alkaline solution, and drying at 212°F . It is of a dark crimson red colour, decomposable before drying by the mineral acids, including carbonic acid, but less readily by sulphuric acid than by the others. When strongly heated it evolves water and oxygen gas. As diluted nitric acid appeared to decompose this salt without the formation and evolution of any but oxygen gas, I selected this acid as the agent of analysis.

Having procured a very thin light flask with a long narrow neck of the capacity of about four fluid ounces, it was about half filled with dilute nitric acid, which, together with the flask, weighed 1862·05 grs., to this 33·16 grs. of the red barytic salt were gradually added, a rapid evolution of oxygen ensued with every addition of the barytic ferrate; at the expiration of twenty-four hours the flask and contents were weighed = 1891·94 grs., which indicates a loss of 3·27 grs. of oxygen gas; the solution was of a light pink colour, showing that decomposition was not wholly effected; but this entirely disappeared, and the solution became colourless on the addition of two drops of weak hydrochloric acid to the warm solution, evidencing so small an amount of the undecomposed salt as not to be worth considering: this solution evaporated to dryness, redissolved, and filtered, gave 2·78 grs. of silica and sulphate of barytes. On the addition of sulphate of soda to the solution sulphate of barytes was precipitated, which, washed, dried and ignited, weighed, exclusive of ash of filter, 24·64 grs.; the oxide of iron precipitated by ammonia gave 8·88 grs., leaving 2·09 for water and loss.

(To be continued)

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The late Eclipse.

At the meeting of the Academy of Sciences, on the 22d ult., M. ARAGO made his promised communication, which has been so anxiously expected, relative to the eclipse of July 8th, 1842, as observed by him and other astronomers at Perpignan.

“M. ARAGO, began by stating that the object of himself and the gentlemen associated with him in the observations at Perpignan, was not so much to verify the accuracy of the calculations as to the precise moment at which the eclipse was to occur, as to determine as far as possible some undecided opinions as to the nature and character of the heavenly bodies on which our earth depends for light and heat; but, being provided with the means of ascertaining the exact moment of the eclipse, they did not, of course, neglect to record it. M. Arago expressed his surprise at having seen it stated by some observers that the phenomenon occurred precisely at the time predicted; for according to his observation, it did not take place until from 30 to 40 seconds later than the prediction. This error of calculation, he observed, might appear to many to be too trifling to deserve notice, but in his opinion, it was inconsistent with the progress made in astronomy, and it would be necessary for the honour of the science to trace its cause and prevent its repetition. The learned academician then proceeded to communicate the result of his observations on the halo which appears to surround the moon after the entire disappearance of the sun, and which modifies the darkness occasioned by the eclipse. Plutarch says—‘The moon in an eclipse of the sun, allows

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a portion of the sun's light to extend beyond her own edges, and thus total darkness is prevented.' The lunar halo is more particularly described by Plantade and Clapiès in their observations of the eclipse of 1706. 'As soon as the sun was wholly eclipsed,' say they, 'the moon appeared to be surrounded by a very white light, forming round the disk of that planet a halo three minutes in width; within this limit the light was the same throughout, gradually failing, and at length dissipating itself in darkness.' The width of this luminous appearance, however, varies according to the eclipse. In 1719 Halley found an extent of two minutes and seven seconds; in 1806 the observation of an astronomer in America gave six minutes. At Perpignan, on the 8th of last month, the width was three minutes and thirty seconds, and it did not vary during the different phases of the eclipse. M. Arago had recommended to his colleagues to make it an important point to ascertain whether the halo had its centre on the sun or on the moon, the existing opinions on this question being of a conflicting nature. Halley and Lonville have affirmed that the centre of the halo coincided with that of the moon; whereas, according to Miraldo and Ferrer, the centre is always that of the sun. The astronomers of Perpignan report that the opinion of Halley and Lonville is the correct one. They measured the luminous coronet with the greatest care, and found it equal on both sides, which led them to conclude that the white aureola, which extends beyond the obscured body of the moon, is not produced by the sun's atmosphere, and is simply a phenomenon of luminous diffraction. The serpentine lights observed on the surface of the moon in 1715, by Halley and Lonville, and which the latter regarded as lightning arising from storms in the atmosphere of the moon at the moment of the eclipse, were not seen at Perpignan. Some meteors, or shooting stars, were, however, observed. It is not improbable, therefore, that the serpentine lights noticed by Halley and Lonville were meteoric appearances brought by chance over the perspective of the superposed bodies. The Toulouse astronomers, in their account of the eclipse of July 8, state that they had observed a luminous opening in the edge of the moon, about forty seconds before the end of the eclipse, and they assign to it an extent of 156 leagues. A similar observation was made, by Admiral Ulloa, in 1778. The luminous point which he perceived on the north-west portion of the moon was, according to him, 109 leagues in length, being a narrow opening or perforation of our satellite, admitting a small portion of the sun's light. M. Arago, without absolutely denying the existence of this opening, states that, in the observations at Perpignan, there was nothing to confirm it. During an eclipse, the moon is designed in black, upon the sun, in its true form. The region of the sun which remains visible is, therefore, limited by two portions of circumference. In the points in which they meet, these two arcs, one dark, the other luminous, form two curvilinear lines which are called *horns*, and which are sometimes very thin and sharp. The

luminous rays of the sun, which define clearly even the summit of the horns and surrounding parts, cross the surface of the moon to arrive at the earth. This preliminary description introduces some remarks by M. Arago, on the important question as to a lunar atmosphere. If, says this gentleman, the moon had a sensible atmosphere, these rays would deviate, the circular form of the sun would be affected, and the horns would show inflections and irregularities. Nothing of this kind were seen at Perpignan. It was only at rare intervals that the horns appeared mutilated, and they were never so completely. The observations on the bright spots of the sun led the astronomers of Perpignan to the same conclusion as to the non-existence of a lunar atmosphere. When the edge of the moon, during the eclipse, passed a solar spot nearest the black disk of the sun, it had the same luminous intensity as the remainder. This equality of light, says M. Arago, would not have existed if a vapor of any kind, even of no greater extent than the distance of the Luxembourg from the Observatory, had surrounded the moon as an atmosphere. The number of stars seen at Perpignan during the height of the eclipse was only ten, from which we may infer that the darkness was at no time great. The accounts given by the ancient astronomers of the eclipses observed by them are very different. According to them the darkness in some cases was more profound than that of night, and the stars shone with a brightness which filled the inhabitants of the earth with admiration and astonishment. It would appear, however, by the accounts of other astronomers who watched the eclipse of the 8th of July, that a greater number of stars were visible than seen by M. Arago and his colleagues. This was particularly the case at Montpellier, and also at Milan, although without the central range of the eclipse. The thermometrical observations of M. Arago are less extensive than many persons could have wished. He is brief in his account of the change of temperature experienced during the maximum of the eclipse. He states, indeed, that the two minutes and a quarter of the total occultation of the sun sufficed to cool the atmosphere to such an extent, as to cause an abundant dew to fall upon the trees and plants, which were dripping with wet when the sun again made its appearance; but he has omitted to state with precision the degree to which the mercury fell in the thermometer. M. Lenthéric, professor of mathematics at Montpellier, explicitly states that at the commencement of the eclipse there, the thermometer stood at 18° centigrade (about 75° Fahrenheit). At the moment of its greatest obscurity, it marked $15\frac{1}{2}^{\circ}$, but at the end of the eclipse, the mercury rose to 20. M. Lenthéric relates a curious fact as to the termination of the phenomenon. The dazzling effect of the sudden return of the light, he says, was such, that he could not, at the moment, distinguish the hands of his chronometer, and therefore was unable to determine the moment with the precision desired. An interesting experiment was made by the Faculty of Sciences of Montpellier, to ascertain the luminous intensity at the different periods of

the eclipse. The means employed was the daguerreotype. All the proofs gave a sufficiently defined image of the phenomenon to enable the members of the faculty to determine, by actual admeasurement, the relative apparent diameters of the sun and moon. At Toulouse, M. Flangerques not only noted down a fall in the temperature of 4° centigrade during the eclipse, but also saw the mercury fall in the barometer. The mercury fell to thirty-one hundredth of millimetres below the height at which it would have stood if the difference of temperature had been the cause of the variation. This depression is, indeed, of itself unimportant, but it nevertheless shows a deviation from the normal action of the barometer, for it is known that this instrument usually goes on rising from the getting up of the sun until nine in the morning when it attains its maximum. M. Arago, in the course of his communication as to the observations at Perpignan, states that during the latter period of the eclipse, he saw on the edge of the black disk of the moon, a short protuberance of fire, two minutes in height, and presenting an appearance like that of the glaciers of the Alps illuminated by the setting sun. At Narbonne, the appearance was that of a distant light-house. M. Littrow, of Vienna, also noticed this protuberance, and gives to it an extent of five minutes, or the twelfth part of a degree. M. Bouvard, of Digne, distinguished luminous points proceeding from the edges of the moon, but he attributes them to divergent rays. There will naturally be much speculation as to the character of the protuberance noticed by M. Arago. Some members of the Academy have already thrown out the idea of a mountain of the sun rendered visible in the atmosphere of that body. The theory of Herschel is, that the sun, which is the source of light and heat to us, and which has been regarded as an incandescent body, is in reality dark and inhabitable. M. Arago, whilst he affirms that the protuberance which he observed is not of the moon—no such discovery having ever been made even with the most powerful telescopes—does not admit that it is a mountain of the sun, not that there is anything repugnant in the laws of science in supposing the existence of a mountain of the sun, 17,000 leagues in height—or, according to M. Littrow's calculation of the extent of the protuberance, 50,000—for objects are only large or small comparatively, and Herschel has shown that the sun, by its prodigious mass, might have mountains, even 120,000 leagues in height; but M. Arago's doubts are founded on the diversity of opinions as to the character of this protuberance. This mountain, if it were one, would have presented a fixed projection and the same angle to each of the observers, which was not the case. M. Arago, therefore, is disposed to regard the phenomenon as one of diffraction. It is proposed, however, to determine this point by experiments with artificial means on the summit of some high mountain. Another curious circumstance mentioned by M. Arago; is the following:—At about the middle of the eclipse, M. Arago was able to perceive the whole disc of the moon. What was the light which enabled him to do this? It

could not be the ash-colored light (*la lumière cendrée*) left by the eclipse, for that is exceedingly feeble. There is, in this fact, a mystery which is perhaps impenetrable in the present state of astronomical science. The effect of the eclipse upon the population of Perpignan, who were watching it, is described by M. Arago as singular and even affecting. The gravest persons were unable to restrain expressions of joy when the sun re-appeared, and, whilst the eclipse lasted, anxiety was depicted on every countenance. At the foot of the citadel in which the astronomers were making their observations, was a regiment of soldiers. They were laughing and full of gaiety until the face of the sun was obscured, when suddenly they seemed struck with dismay and stupor. The effect upon animals was so remarkable, that, if some portion of what is related did not rest on such good authority, it would not be credited. One of the friends of M. Arago had placed five healthy linnets in a cage. During the sudden darkness of the eclipse three of the five died. The oxen formed into a circle, with their horns thrust forward, as if preparing for the attack of an enemy. At Montpellier, bats and owls left their retreats, and sheep laid down as for the night, and the horses in the fields were in a state of terror. In addition to these facts, it was stated to M. Arago, in the Academy, on the authority of M. Fraisse, a distinguished naturalist, that a swarm of ants in full march stopped short at the moment of occultation.—*Galignani*.

On the Purification of Fish Oil. By MM. GIRARDIN and PREISSER.

From the "Bulletin de la Société d'Encouragement," for July, 1842.

The constantly increasing price of seed oils (*huiles de graines*) has drawn the attention of speculators to whale oil; and those who first thought of mingling the latter with vegetable oils, for purposes of illumination, have realized large profits. It is now difficult to find the oils of colza, &c., entirely free from fish oil.

In various scientific and technical works, we find processes for the purification of fish oils, which although simple, are useless, and rather tend to mislead those engaged in their sale, or purification.

Thus, Mr. Davidson, of Edinburgh, purifies oil by treating it with one per cent. of chloride of lime, diluted with water, under violent agitation; and he assures us that the odor is entirely destroyed, but we obtain only a bleached and thick matter, which is clarified by adding 85 grammes of sulphuric acid, diluted with sixteen or twenty times its weight of water. The mixture is stirred, gently boiled, and, after filtering warm, is suffered to cool and repose for several days. MM. Girardin and Preisser repeated this process without any satisfactory result.

The "Journal hebdomadaire des Arts et Metiers" points out several processes for the same purpose. The first consists in mingling 28 gm. pulverised chalk, and 42 gm. slaked lime, with a gallon of the oil, stirring well, and adding 0.236 litre water; after two or three hours of repose, it is mixed again, and this operation repeated for

two or three days ; 28 grm. of common salt, dissolved in 0.710 litre water, is then added, the mixture stirred at intervals for two days, suffered to settle, and the oil drawn off.

Another process in the cold, applicable to cod oil, consists in putting into $4\frac{1}{2}$ litres of the oil, previously prepared by the preceding process, 28 grm. of chalk ; then, after 24 hours, 28 grm. of potassa, dissolved in 113 grm. water ; and finally, after several hours, 57 grm. common salt, dissolved in 473 grm. water. After settling a few days, the oil is drawn off.

Neither of these processes is sufficient, as MM. G. and P. have satisfactorily ascertained. The same journal asserts that the oil is obtained so pure by the following process, that it can be employed in woollen manufactures.

Put into $4\frac{1}{2}$ litres (1 gallon) of impure oil, 35 grm. chalk, an equal amount of slaked lime, and 0.473 litres of water ; after stirring, and a repose of several days, add 0.473 litres water, and 85 grm. potassa ; heat the liquid without bringing it to boiling, and draw it off when the oil has a light amber colour ; it has now only a pungent, fatty odor : finally, add 0.473 litres water, containing 28 grm. salt, and, after boiling the mixture for half an hour, turn off the oil into a reservoir. This process does not refine the oil.

Many English patents for the same purpose were tested by Messrs. G. and P.

One treats fish oils in the cold by bone black, in small fragments, and filters through animal charcoal, after repeated agitation. Such a process clarifies the oils, and removes a portion of their empyreumatic odor, but does not in the least diminish their essential odor.

Another method, recently published in France, has succeeded no better. It consists in pouring into the oil a solution of bichromate of potassa, mixing thoroughly, then adding a solution of oxalic acid ; the action is energetic, but, after repose and drawing off, the oil still retains its characteristic odor.

There is a process among the French patents, which consists in heating the oil merely to simmering with ten parts of water for five or six hours, and, towards the close of heating, adding a milk of one part of water, with one-twelfth of chalk, and one-twelfth of lime. After settling perfectly, it is drawn off and run into reservoirs, through carded wool or pounded charcoal. This process clarifies the oils, but decolorizes them imperfectly, and does not at all remove their odor.

At Rouen, they refine whale oil by sulphuric acid, as in operating on seed oils ; but this method removes neither color nor odor. If, previous to this operation, it be stirred for some hours with chalk, and a current of steam be passed through it, a bleached liquid obtained, which by the addition of a suitable quantity of sulphuric acid, deposits plaster on settling. The clear oil, filtered through animal black, has lost a portion of its deep color, and has not a strong odor ; but it is not perfectly purified, even after many successive filtrations.

The oxygenation of oils leads to very bad results. Messrs. G. and P. remark, that oils filtered and treated, whether by chlorides, lime, chalk, or animal charcoal, and then left to themselves for thirty or forty days, deposit a bleached organic substance, soluble in water and ether, analagous to margarine, and, while depositing, the oil is more and more decolorized. Fish oil may be obtained, of a quality resembling fine olive oil in appearance, by exposing it to the sun, then to the action of chloride of lime, and filtering several times through animal charcoal. The odor is lessened, but not entirely removed,

A simple exposure to the sun for several months determines an abundant deposit, while the oil is clarified and sensibly purified.

If whale oil be brought in contact with caustic ley, employed cold and in small quantities, the decolorization is hastened; the mass separates into two distinct strata—the upper one, decolorized, is very fluid and limpid, but always odorous; the lower, which is very small, is a mixture of the alkaline solution, strongly colored brown and of all the solid portion of whale oil analagous to margarine. It is not necessary to submit the decanted oil to any other process of purification; in this state it is suitable for all manufacturing purposes, excepting on account of its odor, which is always well defined.

It appears from the experiments of Messrs. G. and P. on fish oils, that we at present possess no sufficiently efficacious means of removing their strong and disagreeable odor. The best method, at present, is to submit them either to the action of alkalies, or to the successive action of chalk, steam, and sulphuric acid; to suffer them to repose, and filter several times through animal charcoal. We thus obtain a clear oil, less colored, and of a less repugnant odor; but its want of odor is out of the question.

The refining and purification of fish oil is the more important, since, for the last twelve years, their importation has constantly increased. Thus, in 1827, there was entered only 3,000,000 kilogrammes, (about 6,000,000 lbs.,) the greater part of which came from the Islands of St. Pierre and Miquelon; while, in 1839, the importations amounted 9,200,000 kilogrammes, representing a value of 5,500,000 francs.—*Journal of the Franklin Institute.*

On the Introduction into Scotland of Granite for Ornamental Purposes, by Messrs. Macdonald and Leslie, of Aberdeen. By Professor TRAILL, F.R.S.E., M.W.S., &c., &c.

THE first idea of employing the refractory, but enduring material, granite, in sculpture, appears to be due to the ancient Egyptians. Those who have enjoyed opportunities of examining their colossal buildings, have acknowledged the precision, and even delicacy, of the figures and ornaments with which that ingenious people contrived to enrich their architecture. Specimens of their sculpture in granite, which have for three thousand years resisted the action of the elements, and the yet more destructive influence of barbarous invaders,

still astonish us by the high polish of their surfaces, and the delicate finish of their details. Even a visit to the Egyptian Saloon of the British Museum will prove that, in accuracy of muscular delineation, and in the communication of absolute *fleshiness* to the lips and features of some of the figures there preserved, the ancient Egyptians evinced a high perfection in the art of sculpture, in a material of the most imperishable kind, on which few succeeding artists have ventured to employ their chisel.

In our own times, the fabrication of slabs, pedestals, and vases, in hard porphyries, and in granite, has been carried to great perfection in Sweden. The quarries of Blyberg, at Elfdalen, for many years, have furnished materials for Swedish ingenuity and skill. The elegant forms and high finish of their works in those refractory materials, have contributed greatly to the splendour of the Swedish capital, and are known and admired over Europe. Yet, though our own mountains yield no less beautiful and durable materials, it is surprising how long we have remained without any attempt to apply them to the purposes of ornamental art. It is true that, for more than half a century, Aberdeen has exhibited a city chiefly built of hewn granite; that, more lately, this same material has been employed in the construction of Waterloo Bridge, in London, and in a few other works; and that Cornish granite appears in the pedestals of a few statues in some of our towns. But the idea of giving a polish, equal to that of ancient Egypt, to our granite in works of considerable size—of introducing this splendid material as a domestic ornament in our halls and saloons, and as lasting memorials of departed worth in our cemeteries—is undoubtedly due to two citizens of Aberdeen, Messrs. Macdonald and Leslie, who carry on extensive works in that town, where the gray granite of Aberdeen, and the rich red granite of Peterhead, are cut into an endless variety of ornamental articles, which receive the highest polish.

A late visit to their establishment convinced me that these gentlemen have reduced to practice the difficult problem of giving any required form to so stubborn a material as granite, and of communicating to its surface an exquisite polish, which shows it to be well suited for domestic ornament, and as a superb decoration for the abodes of rank and opulence. The rich warm tint of the Peterhead granite, in particular, will harmonize better with the gilded ornaments and gorgeous hangings of a modern gallery, or superb saloon, either as tables or as pedestals for works of art, than furniture made of the most costly woods, or even than the snowy marble of Carrara.

For monumental work, this enduring material possesses advantages over the best marble. In our climate, the effects of rain, sudden frosts, and succeeding thaws, are soon perceptible on Carrara marble, or any other kind, exposed freely to the weather. Marble thus soon loses its glossy surface: it contracts greenish stains from the vegetation of minute *Byssi*; and inscriptions, in a few years, from these causes, become illegible. The polished granite of Aberdeenshire

retains its polish most perfectly under all atmospheric changes, does not contract any stain from vegetation, and unless wantonly mutilated, will transmit the inscription engraven on it to distant ages. The sharpness of the Egyptian hieroglyphics, carved in a very similar rock, three thousand years ago, at this day, proves the durability of granite carving. A beautiful cenotaph of red granite, from the works of Messrs. Macdonald and Leslie, has been exposed to all the vicissitudes of our changeable climate, for six or seven years, in the churchyard of Falkirk, and appears in the full lustre of its original polish, as if it were erected yesterday.

Fine specimens of granite monuments, by the same artists, may be seen in the noble new cemetery at Glasgow, which are chaste in design, beautiful in execution, and seem calculated to bid defiance to every destroying influence, except wilful injury.

On visiting the establishment of Messrs. Macdonald and Leslie, at Aberdeen, I saw several finished specimens, and many works of this material in progress, as I was conducted through the different departments, by the intelligent and most respectable head of this interesting and new employment of national art and industry.

The gray granite is of a close grain, and contains more mica than the red. It is brought from quarries on the Dee, a short way above Aberdeen. The red granite is of a larger grain, abounding in felspar and in quartz, intermingled with small specks of mica, and bears a strong resemblance to the syenitic rock of which the finest ancient Egyptian monuments are fabricated. This comes from the vicinity of Peterhead, and is brought by sea to the works. Both are susceptible of a fine polish, which they retain unimpaired by the weather. Blocks of almost any size may be obtained free of flaws, or imperfections. In the sawing room, several blocks were then under the machines, which are moved by a 14-horse power steam engine. I observed one block, ten feet long, cutting into six or eight slabs. The saws are, as usual in such works, of soft iron plates, secured in a frame, and operate on the stone by means of quartz-sand and water, applied as in slicing marble. No emery is requisite in these operations, the particles of siliceous sand being sufficient to cut the quartz, the hardest material in the granite. Frequently, fourteen saws are used in a single frame, and occasionally they have had as many as eighteen employed at once on a single block of stone. The progress of the work, of course, is slow; it requiring a whole day to cut a groove, two thirds of an inch in depth, in the granite. The slabs, when cut, are polished by moving one over the other, by appropriate machinery; siliceous sand being first interposed, and then emery, of various degrees of fineness, until the requisite degree of lustre is obtained.

The first dressing of the granite blocks into parallelopipeds, cylindrical masses, or other curved forms is performed by *hand-picks*, with short handles, and heads about four pounds in weight, which the workmen, from long habit, wield with surprising accuracy. The

surfaces are then reduced to a regular form, by means of well-tempered chisels, urged by iron mallets; the chisels require a very particular temper, which must be neither very hard nor very soft, else they would either lose their edge by *chipping*, or fail to cut the stone. I observed that they frequently require sharpening in the more delicate kinds of work. The chisel is held by the workman very obliquely to the surface of the stone, and he separates very small particles at a time.

I have already described the polishing of plane surfaces. Circular forms, such as *stellæ*, frusta of columns, as pedestals for busts, vases, and the like, are fixed in well-contrived lathes, and are whirled round by machinery, while the sand and emery are applied to their surfaces by means of thick plates, or bars, of iron, previously forged to their various curvatures, when they are not cylindrical.

I saw a large vase, about four feet in diameter, prepared by the chisel for the process of polishing. Its graceful curves were beautifully and accurately cut by the chisel; the iron bars, 1 or $1\frac{1}{4}$ inch in thickness, neatly forged to its various curves, lay beside it, ready to be applied when it was fixed in the lathe.

In the warerooms were many finished articles of great beauty and elegance, such as well-executed pedestals for busts, or vases, of red and gray granite; chimney pieces of the same material, numerous slabs, tables, and seats for halls, and beautiful vases, in a considerable variety of forms, rivalling those of classic Italy in shape, mural tablets for monuments, and some alter-formed tombs of magnificent size. These last were made to order. Some of the chimney pieces are intended for the Earl of Lauderdale's residence, Thirlstane Castle, and some of the slabs for Sir Robert Peel, &c., &c.

I was surprised at the neatness of the *lettering* on all the monuments, and saw the men at work. The monument is first finished in other respects; the letters are carefully traced with a dark, or light, crayon, according to the colour of the stone, and the workman traces the outline of the letter on the stone by light strokes of a fine-edged chisel, held nearly vertically; deepens the lines by a succession of similar blows, while the chisel is held very obliquely, removing the stone in a state of powder, so as to avoid chipping. Roman capitals are thus easily formed; but I saw old English, or German, letters, with a superfluity of curved lines, carved on the granite with equal precision.

But the most remarkable work which I saw in this establishment, was the neatly finished statue of the late Duke of Gordon, intended to be erected in one of the streets of Aberdeen. It is 11 feet high, of a single block of granite. This statue was modeled by Mr. Thomas Campbell, the sculptor, and has been transferred from the model to the granite by Messrs. Macdonald and Leslie. Two men were at work on the drapery, at the period of my visit. They worked with fine chisels, held very obliquely, and urged on by iron mallets of two or three pounds in weight. The attitude of this statue is

simple, and the features are said to be very like the original. This, which may be considered as the first specimen of a British statue of a single block of granite, in emulation of the durable monuments of ancient Egypt, is a memorial by the county to the late noble and gallant officer, and, when erected, will be a distinguished ornament to Aberdeen.

Another great public work, executed by the same artists, is already erected in that town. In 1842, the splendid public markets of Aberdeen, excelled by none in Europe in elegance, were opened. The great saloon, containing the fruit and vegetable market, a magnificent hall, 300 feet in length by 100 feet in breadth, has within it a noble fountain of highly polished Peterhead granite. An octagonal basin, constructed of polished blocks, stands about one-third the length of the hall from the southern extremity. From the centre of this basin rises a shaft, ten feet high, supporting two circular cups, or shallow vases, one placed over the other. The lowermost is formed out of a single block, seven feet three inches in diameter, and the upper has about half that width. A constant jet of water rises from the centre of the upper cup, flows over its edges into the lower vase, which also overflows, in a thin sheet of limpid water, into the basin below, whence water is drawn for all the purposes of the market. I have seen no fountain in Britain so fine as this. It resembles in form, and surpasses in material, the finest fountains I saw in Spain; yet it was erected by Messrs. Macdonald and Leslie for £200.

The same artists are at this moment engaged in executing a similar monument for Lord Prudhoe, which, I understand, will cost about £200.

Indeed, considering the difficulty of working so hard a material I was surprised at the moderation of their prices, for articles produced at their interesting establishment.

For instance :—

1. A hall table slab of polished granite, measuring 4 feet long by $21\frac{1}{2}$ inches wide, costs £4 15s.

It may be stated, that slabs may be furnished, of any required size, for from 12s. to 14s. for each square foot of surface.

2. Pedestals for busts, square or columnar, with plinth, and an ovolo when columnar, of the usual size, for £10.

3. Mural monumental tablets, with vase, trussets, &c., from £6 to £9, according to the size.

4. Mural tablets, with base, cornice, and pedimented top, from £10 to £12.

Lettering, of the usual size, is charged 4s. 6d. per dozen of letters.

5. An elegant *Tuzza-formed* vase, of classic shape, 4 feet 9 inches in diameter, and standing 2 feet 9 inches high, on a beautiful pedestal, costs £40.

6. They have also executed columns of granite, for halls and vestibules, at prices equally reasonable, in proportion to the size and style of decoration. But of all the purposes to which they have

hitherto applied the granite, it seems especially suitable for monuments of every kind, both from the beauty of the highly polished material, and its imperishable nature under all vicissitudes of the weather.

The extent and perfection to which these gentlemen have carried the working of this very refractory, but beautiful, stone, may be considered as forming an era in British art, and require only to be more generally known, to be appreciated and encouraged by public taste and munificence.

Edinburgh, March 18, 1843.

Edinb. New Philos. Jour.

Report by MAJOR GENERAL PASLEY, on the Breaking of Axles, and other Causes of Accidents upon Railways generally.

*Railway Department, Board of Trade,
Whitehall, 23d December, 1842.*

MY LORD,—After the unfortunate accident which took place on the 8th inst., on the London and Birmingham Railway, I went to the Vauxhall terminus of the South Western Railway, to inquire into an accident, unattended with personal injury, which took place on that railway on the 10th inst., from the breaking of a crank-axle of one of their six-wheeled locomotive engines. On inspecting the fractured iron, it seemed to be of good quality, but had been made in an improper manner, by welding flat parallel plates together, contrary to the usual and more perfect custom. At that station I had an opportunity of comparing six-wheeled and four-wheeled locomotive engines, and I have made the same comparison since on the Croydon Railway; and I now beg leave to submit to your lordship the result of my observations and reflections upon railway accidents, whether depending on the construction of the locomotive engines, or other causes.

1. *The usual construction of locomotive engines compared.*

Four-wheeled engines, with inside bearings, are exclusively used on the London and Birmingham Railway, in compliance with the opinion of Mr. Bury, the superintendent of the locomotive department of the company; and, as the axles of those engines had often been broken without any disastrous result, and that railway (which is extremely well managed) had been as free from accidents, if not more so, than any other; I did not feel myself warranted in making any official remarks in depreciation of four-wheeled engines, until the recent accident of the 8th instant. In that accident, the fore axle of one of their four-wheeled engines broke off transversely close to the inside of the nave of the wheel, so that the wheel was thus, as it were, entirely cut off, and got away from the engine, which, therefore, by having three wheels only, lost its former direct motion, and moved round with a circular motion, which threw it off the rails and carried it down an embankment, where it rolled over and was found

with its head reversed, that is, pointing in a contrary way to its original course, so that it must have described a semicircle before it stopped. Further particulars relating to this accident were reported in my letter to your lordship of the 13th inst.

In conversing upon the subject with Mr. Bury, and with Mr. Creed, the Secretary of the London and Birmingham Company, these gentlemen both declared their opinion that the same would have happened to a six-wheeled engine, under the like circumstances; but there were none but four-wheeled engines on that railway, so that I could not make a personal comparison of the two sorts at the period of the conversation alluded to. Having since done so with great attention, as before mentioned, and having allowed myself ample time to consider the question, I think it my duty, with due respect for those two gentlemen, to record my dissent from their opinion.

In the case of any of the axles of a four-wheeled engine, of the London and Birmingham Railway pattern, snapping off short close to the inside nave, the wheel must necessarily get away altogether, there being no outside framing to confine it.

In the case of the fore axle of a six-wheeled engine with outside bearings breaking, on the contrary, as the journals are outside, the fracture must necessarily be between the bearings; in which case the experience, not only of six-wheeled, but even of four-wheeled, engines proves that no serious danger is likely to result. It has been urged, and I believe justly, that, when an axle breaks between the bearings, the wheels are more deranged, and more liable to leave the rails, by the outside, than by the inside, mode of fixing; but the fore wheels of a six-wheeled engine with outside bearings cannot get away altogether under any circumstances, unless an axle were to snap short off in two places at once, close to, and on, each side of the nave of one of these wheels, which is impossible. Setting aside, therefore, the supposition of a double fracture, if the fore axle of a six-wheeled engine breaks, no matter where, the wheel is confined closely between the strong frame of its outside bearings and the smoke box, which is also sufficiently substantial, so that it cannot possibly disengage itself entirely. Hence the train may be retarded and stopped, but the engine can neither break down, nor be thrown violently off the rails; in short, no accident injurious to the safety of passengers is likely to occur. If the crank, or centre, axle of a six-wheeled engine should be fractured, the fore and hind wheels alone are capable of supporting the whole weight of the engine, so that it cannot break down. If the hind axle of a six-wheeled engine should break, and even supposing an impossibility, namely, that both the hind wheels should come off, no result injurious to the public safety can be apprehended, because the centre of gravity of locomotive engines of this description is a little in front of the centre axle, and therefore the engine will still remain in the satisfactory state of stable equilibrium.

Having thus stated my opinion, and the grounds on which I have formed it, as to the superior safety of six-wheeled engines, I beg leave to report on the nature of accidents likely to be attended with danger to railway passengers.

2. Collisions of trains, or locomotive engines.

Collisions of trains meeting each other, which are the most dangerous, cannot take place on railways having double lines of rails, if the switches are self-acting, and such that no train going at full speed can quit its own line, on coming to the points where the crossings are laid out, by any carelessness of the engine-driver.

Collisions from one train running into another from behind, cannot possibly be prevented by any mechanical means. If the regulations of the railway company are good, and their enginemen are skilful and possessed of judgment and presence of mind, such collisions cannot happen, except in the case of intoxication. Sobriety is therefore indispensable in this class of men, for, without it, all other qualifications go for nothing.

3. The breaking down of locomotive engines, or railway carriages.

If a locomotive engine, or its tender, breaks down at the head of a train, the carriage immediately in the rear of the tender suffers the most, and the second and third carriages also may be more or less damaged. Generally, in cases of this kind, the carriage next to the tender is dashed to pieces, or nearly so; and the passengers, when any have been in it, have been killed, or severely injured, whilst those in the next carriage have either escaped or been less injured. Hence the expedient of placing an empty carriage of some sort next to the tender has been generally, and I think justly, deemed conducive to safety. A truck, loaded with hard materials, such as iron, stone, &c., placed next to the tender, might, on the contrary, do infinitely more harm than good. It has been suggested that a peculiar carriage, with more powerful buffer-springs, or, as it were, all buffer-springs, would be desirable, instead of a common carriage, and I believe so; but it is still more important to adopt such measures as shall do away with the risk of locomotive engines breaking down, or being suddenly stopped by some obstacle capable of producing that effect.

4. Slips in cuttings and embankments.

These have invariably arisen from the slopes having originally been made too steep, perhaps for want of experience on the part of railway engineers, who had, as it were, to create a new art, for they have never in the first instance allowed a slope of more than two to one, even in the worst descriptions of soil, which I should myself, when new to the subject, have considered ample; but from subsequent experience and observation, I am of opinion that, in all deep cuttings in plastic clay, slopes of four to one on one side, and of three to one on the other, ought to have been allowed; for, though both sides are liable to slip in such soil, yet one side is usually more exposed to this injurious action than the other, owing to the inclination of the strata. But it may be doubted whether, in all cases, inexperience on the part of railway engineers could have been the cause of their adopting insufficient slopes; because, in Sir Henry Parnell's book on road-making, published so far back as

1833, it is laid down as one of the rules, &c., professedly derived from the practice and experience of the late eminent civil engineer, Mr. Telford, that in the London and plastic clay formations, it will not be safe to make the slopes of embankments, or cuttings, that exceed four feet in height, with a steeper slope than three to one. As most of the great railways have been executed since that period, in some of which the depths of cuttings are from 15 or 20, and even 70, feet, and the heights of embankments also very considerable, a query occurs, whether inexperience could have been altogether the cause of neglecting so salutary a maxim. May not, in some cases, the apprehension of deterring the public from subscribing to a new railway, have caused a steep slope to have been adopted in preference to a flatter one, because the latter would have made the estimated expense of the removal of earth much greater? However that may be, experience has shown that, after embankments have once come to their bearings, slips are not to be so much apprehended as in deep cuttings. In the latter, laid out, as before observed, with slopes not exceeding two to one, experience has proved that slips must be expected for years after the opening of the railway. The repeated slips on both sides of the Croydon Railway, for example, took place nearly two years, and that on the west side of the Bugbook cutting on the London and Birmingham Railway, more than four years, after these lines had been opened; and in both cases, notwithstanding that both lines of rails were completely buried by the immense masses of earth thrown down, yet no danger to passengers resulted, owing to these cuttings having fortunately been carefully watched. This precaution, therefore, of continued and careful watching, is absolutely necessary in respect to embankments and cuttings in plastic clay, especially the latter; for, if not discovered in time, a very insignificant slip of earth from one of the sides of a cutting, though capable of being removed in a few hours, might, by suddenly stopping an engine at full speed, endanger the lives of the passengers in the carriages nearest to the tender—an instance of which actually occurred on a different railway about a year ago. But if the state of cuttings and embankments be always carefully attended to, no serious accident is to be apprehended from any slip, however considerable. I shall only further observe, that it is well known that not merely the nature of the slopes, but also the proper drainage of cuttings and embankments, are essential considerations, both in preventing slips in the first instance, as well as the repetition of them in unfavourable soil.

5. Of the axles and wheels of railway carriages.

The breaking of an axle of a locomotive engine is, I believe, more common than the public generally suppose, and, though seldom fatal, it sometimes may be so, as in the case of the accident of the 8th inst. on the London and Birmingham Railway; and though the fracture of an axle of one of their four-wheeled locomotive engines, in that peculiarly dangerous manner, has only occurred once in six

years, yet, admitting the possibility of a precisely similar fracture of any of their axles occurring again, the same result must inevitably follow ; but I see no mode of guarding against it in four-wheeled locomotive engines with inside bearings, except by adding a light outside framing on each side, which need not have regular braces like those attached to the present inside frame, nor need they act upon the axle when the engine is in a perfect state ; but, though not in contact with it, they should be so near as to come into play whenever the axle breaks. By this arrangement the wheels would not be able to get away under any circumstances. I have seen a similar arrangement adopted on the South Western Railway, in respect to the fore axle of one of the six-wheeled engines with outside bearings and journals, by John V. Gooch, their superintendent of the locomotive department, who has attached horns, as they are technically termed, inside of the wheels, ready to act should the axle break, but not otherwise ; a precaution, however, which, though it may be prudent, is not absolutely necessary in the six-wheeled engine. I have also seen the crank axle, or centre axle, of a six-wheeled engine made with double bearings, springs, and braces, both in one of Mr. Robert Stephenson's old engines, and in one at the South Western Railway, and also in Sir John and Mr. George Rennie's new engines ; but, on the whole, after having carefully considered this subject, I came to a conclusion that six-wheeled engines, with outside bearings for the fore and hind axles, and with inside bearings for the crank axle, would be a convenient and sufficiently safe arrangement, especially if horns of the description before mentioned be used as a sort of guard for the fore axles in the event of a fracture. After having formed this opinion, I was informed that Mr. Gray, of the Hull and Selby Railway, whose engines I have not yet seen, had previously adopted and actually carried it into effect. Under these impressions, I do not think that Mr. Robert Stephenson has made a change for the better in his new patent six-wheeled engines, in which he has adopted inside bearings for all his axles. I also consider the want of flanges on his centre wheels (which is a peculiarity in that gentleman's engines) to be rather a defect than otherwise.

6. Hollow axles, &c.

The axles of the engines and carriages of railways are usually tested in a manner more or less severe, and those which fail are rejected ; but it is believed that, though good at first, they become deteriorated in time, not merely by wear, but that the metal gradually loses the proper fibrous texture of malleable iron, and becomes crystalized and brittle like cast iron. Besides magnetic influence between the brass journals and the iron, which has by some been considered the chief cause of the deterioration, the axles of railway engines, &c., are liable to two sorts of injurious action ; first, the jolting or jumping at high speed, owing to little inequalities in the rails, which is equivalent to cold hammering ; secondly, to torsion in going round curves, or otherwise. In travelling upon six-wheeled

and on four-wheeled engines, I have found the former generally smoother, so that the action of percussion appears to me to act with greater violence on the axles of the latter. Mr. John Oliver York has recently taken out a patent for hollow axles, in which only two semicircular plates of malleable iron are welded together, instead of a great number of bars being united by the same process, to form a solid axle, according to the usual custom. His opinion is, that it is the original welding chiefly that injures the iron; but whether his theory be correct or not, I have been informed that, on comparing his hollow axles, of 4 inches exterior diameter, with solid axles of $3\frac{1}{2}$ inches diameter, by subjecting both to the blows of a 38-lb. hammer, the former were found to possess much greater resistance, though containing rather less iron. If the hollow axle should show the same superiority over the solid one, in its resistance to torsion, as it has evinced in respect to percussion, (an experiment which is about to be tried at the Camden Town station of the London and Birmingham Railway, at the suggestion of Mr. Bury, who considers the former to be the most injurious action of the two,) I think it probable that hollow axles will generally be adopted by all railway companies, as soon as their present solid axles shall require to be replaced by new, which must of course be done sooner or later. In respect to the working, or cranked, axles of locomotive engines, the hollow pattern is entirely inapplicable, but it may be used for all the other axles of locomotives, and all the axles of tenders, carriages, waggons, and trucks upon railways, without exception; and upon the whole, I consider it very promising.

7. Of railway signals.

These are nearly assimilated on all the railways that I have inspected; the red colour, or light, being the signal of danger, requiring a train to stop, and the green being the signal of caution; but in some railways, the red light in the rear of the hind carriage of a train is placed so low that it may be obscured by some intervening object, and consequently not seen by the engineman of another train, or of a special engine, following, which was the cause of a collision on the Croydon railway. It is therefore desirable that all railway companies should adopt the system, of which the principal ones have already shown an example, by placing on the hind carriage of every train two red lights, one on each side, so high that they cannot be obscured; and, on the lines alluded to, the low central red light has also been retained, though less necessary. In fogs, a red light is sent out from a station to a sufficient distance to warn the coming trains to slacken their speed; and in the case of a train breaking down, or being obliged to stop, at some intermediate point between two stations, a man is sent back with a red light as a warning to the next coming train to stop, in order to prevent collision. On the London and Birmingham railway, a very ingenious plan has lately been adopted in case of fog, or danger; that is, to send a man to place a small tin box, containing a charge of gun-

powder, with a little fulminating powder, on the line of rails by which the next train is advancing; as soon as the wheel of the locomotive engine of which passes over this box, it fires the charge, with an explosion sufficiently loud to be heard in the most stormy night, but not powerful enough to injure the rails, or engine. Under the circumstances supposed, this arrangement, as a night signal of danger or caution, is evidently preferable to a red light, because no neglect or inattention on the part of the engineman, stoker, &c., can render it possible for them to pass on without being aware of the explosion, which can never fail to take place; but the combination of both might, perhaps, be still better than either of these expedients singly.

8. *Of untried inventions for preventing accidents on railways.*

A great number of plans of alleged improvements in railway engines, carriages, and signals, and in the rails, switches, &c., &c., with a view to safety, as also for preventing slips in cuttings and embankments, have been brought under my notice in the course of the present year, (many of them extremely ingenious, but some very complicated,) by their respective inventors, to whom I have invariably returned the same answer, namely, that I shall be happy to see their inventions, for the sake of personal information, but that I decline recommending them, or even giving an opinion upon them, as the adoption of such plans must depend entirely upon the directors of railway companies, upon whom the Board of Trade will not urge any experimental arrangements involving expense.

I have, &c.,

C. W. PASLEY, Major General,
and Inspector General of Railways.

The Right Hon. the Earl of Ripon, &c. &c.

(*Railway Mag.*)

On the Mode of Calculating the Strength of Cylindrical Steam Boilers. By B. H. LATROBE, C. E.

To the Committee on Publications:

GENTLEMEN,—In the January number of the Journal is an article by Thomas W. Bakewell, Esq., in which the memoir of the explosion of the steamboat Medora, prepared by me at your request, and published in November last, is referred to as “another instance of respectable authority to a prevailing and dangerous error in estimating the capabilities of cylindrical boilers to sustain a given pressure of steam.” I have read Mr. Bakewell’s remarks with respectful attention, and a desire to discover the error (if any) which he alleges to exist in the mode of computation which I had adopted, and for the truth of which, as he acknowledges it to be the “received rule,” I can be under no special responsibility. I am unable, however, to perceive wherein lies the supposed mistake in the formula

which I used, and which is expressed by the equation $x = \frac{2 P t}{D}$;

where P is the cohesive force of the metal of the boiler per square inch— t , the thickness of the shell in inches—D, the diameter of the boiler, also in inches—and x the extreme pressure, per square inch, that it will bear. I now offer the following remarks as expressing my confirmed conviction, with the reasons therefor, that the rule is right, Mr. Bakewell's belief to the contrary notwithstanding.

Let us look into the conditions of the question. In what manner does an elastic fluid, pressing equally in every direction within a hollow cylinder, act to produce its rupture? Plainly by tending, in the first instance, to drive before it, in a radial direction, perpendicular to the surface of the cylinder at any point which may be selected, each particle, or rather line of particles, forming its thickness at that point. This is the primary action of the pressure upon each individual atom of the shell. But it is manifest that the forces tending to separate any two of those atoms contiguous to each other, are not identically those just named, but a *resultant* from them ; for they themselves operate on those two adjacent particles, in lines making so small an angle with each other as to be, in effect, parallel ; so that the particles would be driven off side by side, instead of pursuing opposite directions, as they in fact do in the case of fracture. Moreover how small soever this angle may be supposed, inclosing, as it is assumed to do, only the breadth of an atom at the periphery of the cylinder, it will, in embracing the same atom, increase its opening as the radii diminish ; so that if the particle were torn asunder directly by these very radial forces, their action would obviously become more powerful as the diameter of the cylinder lessened, which is in opposition to the admitted fact that, the less the diameter, the *greater* the strength of the vessel. What, then, is the resultant of these radial forces which has just been referred to, and of which we are in search ? In seeking an answer to this question, we must bear in mind that, in tearing a body of any kind asunder, two forces, in some degree superior to its cohesion, must be exercised upon it in *opposite* directions ; otherwise (unless the two were in exact equilibrium) it would move *bodily* in the direction of the greater force. The two halves of the cylinder in question are thus urged with equal energy, by the outward pressure of the fluid, towards opposite quarters, and we have only to consider the strains to which the shell is subjected at the two ends of any one of its diameters. Now, these strains are equal to the sum of those of the components of all the radial forces on the semicircumference which act tangentially to the cylinder at the points strained, and they are to be estimated by multiplying the unit of pressure by the *diameter* ; for the sum of the radial pressures being represented by the semicircumference to which they are perpendicular, the diameter will represent the sum of those perpendicular to itself, and the radius that

of those parallel thereto, according to the parallelogram of forces—each radial line of pressure being resolved into one parallel to the diameter, and unproductive of effect upon the parts strained, because its direction is at right angles to that in which they must move in separating, and the other perpendicular to the diameter, and productive of motion in the particles subjected to the stress, because its direction is the same that they would severally take in parting from each other. It is, indeed, most manifest, without the aid of rigorous mathematical demonstration, that no force, oblique to the direction pursued by the body to which it is applied, can operate with its full intensity upon that body, but must expend a portion of its energy upon some other object. Mr. Bakewell's error seems to me to consist in losing sight of this elementary truth, for, in using the semi-circumference instead of the diameter in his estimate of the pressure at the extremities of the latter, he omits the resolution of the radial forces into their operative and inoperative elements, and assumes them to act with their full intensity to tear the cylinder asunder, notwithstanding the greater, or lesser obliquity of every one of them, excepting the single central one perpendicular to the diameter. But, it may be said, what becomes of the other components of these radial forces, which act *parallel* to the diameter in question? Are they to be lost sight of, and do they produce no increase of strain upon the points under consideration? for Mr. B.'s theory seems to be, that the cylinder does sustain from them a stress at the ends of the diameter, “in the disadvantageous manner of a string stretched horizontally, and bearing weight.” They are certainly not to be forgotten, though as surely do they produce no such increase of strain. For, in the first place, it must be borne in mind that the body under consideration is a *cylinder*, every point of whose circumference is like all the rest, and that the two points in question, at the ends of any one of the infinite number of diameters, are supposed, for the time being, to be the *peculiar* seats of the stress, only for the sake of the argument and its illustration,—that all other points of the cylinder are precisely alike in their positions and functions. The central line of pressure just mentioned as perpendicular to the assumed diameter, and as producing the maximum of all the radial strains when they are referred to that diameter, is itself a part of a diameter which may, in its turn, become the subject of a similar assumption, and, when it does so, becomes the line of *least* (as it was before of *greatest*) pressure—that is, the line of no pressure at all. In the second place, the *parallel* unite with the *perpendicular* components of the radial forces to give the cylinder the *stiffness*, the supposed want of which is a main difficulty with Mr. B. It is the uniform tension of the steam which gives the required rigidity to the cylindrical vessel, however flexible its material; and if it were of gold leaf, or gossamer, it would still be perfectly stiff for the purpose in view, for its shape would continue *unaltered up to the moment of rupture*; and this preservation of form is all that absolute inflexibility in the shell, *per se*, could accomplish.

I have thus far argued the case upon the simple elementary principles which seem to me to govern it. I might proceed to give an analytical, or geometrical solution of it, accompanied by diagrams; but as this would extend my communication beyond desirable limits, I will refer, instead, to the brief but satisfactory demonstration of Professor Barlow, in the part of his work upon the "Strength of Materials," under the head of "Strength of hydraulic presses and water pipes," (pages 205 to 210, London edition.) This well known treatise is probably accessible to most of your professional readers. It will be seen therein that the learned Professor upholds the received rule, and makes no mention of the existence of a doubt of its accuracy, among scientific men. It is true, he shows that, in consequence of the difference between the interior and exterior diameters of every hollow cylinder, the strain is not uniform throughout the thickness of the shell, but diminishes regularly from the inside to the outside thereof—so that, by the law, "*ut tensio sic vis*," the metal being unequally strained in different parts of its section, the full resistance of all the fibres is not brought into action at the same moment; so that a deduction from the *actual*, in the calculation of the *effective*, thickness, is due to this cause. But the correction on this account, for cylinders of large diameter, is so trifling, that it need not be considered in the present controversy. Thus, where r represents the (internal) radius, and t the actual thickness of the shell of the cylinder, the *effective* thickness is expressed by $t \times \frac{r}{r+t}$; which, in the case

of a boiler of 132 inches diameter, like the Medora's, would make the effective only 0.0061 of an inch (or about $2\frac{1}{2}$ per cent.) less than the actual thickness, the latter being one-fourth of an inch.

The example by which Mr. B. illustrates his views of the theory of strength in a cylinder, although ingenious, seems to me inapplicable to the case, because it is inconsistent with the essential characteristic of the vessel, viz., its equality of resistance everywhere, resulting from the circularity of its form. It is not then allowable, even in argument, to suppose it to be cut at any point, and the cohesion (thus destroyed) of the material composing the shell to be represented by bolts acting as ties, unless we suppose those ties to take hold, not, according to Mr. B.'s supposition, of only two, or four, or more insulated points, but of *every point of the periphery*; in which case, the argument drawn from the want of *stiffness* would fail, and the vessel would be, in effect, restored to its pristine condition of an undivided hollow body, equally strong everywhere; so that the reasoning would be as circular as the cylinder, and would come back by as roundabout a course to its point of departure.

It is necessary to use these hypothetical modes of reasoning with caution; nevertheless, I will offer an illustrative instance, which, I think, will make more clear the correctness of the positions already established upon abstract principles. Suppose that, instead of steam within the cylinder, (which, as Mr. B. has done, I will imagine to be

a ring of an inch in width, and, consequently, giving a square inch for every inch in the length of its periphery,) we should introduce a solid disk of some hard body, fitting close against the interior of the ring. Let us then divide this disk into two equal parts, by cutting it through, on one of its diameters; let us further introduce a wedge between the halves, and force them apart by a power equal to that of steam of any given pressure, operating on the number of square inches contained in the length of the diameter—or, better still, let us introduce steam itself into the fissure, so that the two sections of the plate are forced asunder in directions perpendicular to the line of separation. The space between them need not be more than a hair's breadth to permit the full action of the fluid. Consequently, its pressure upon the ring at the ends of the opening will be on a mere line an inch long, and will produce no appreciable strain at those places. The only tension felt thereat will then be perpendicular to the line of the opening, and strictly tangential to the curve, and will be expressed by the number of inches in the diameter multiplied by the steam pressure per square inch. The ring might, indeed, give way indifferently at any point of its circumference, were it not that it has, at all other points than those at which the disk is cut, the benefit of its *friction* upon the latter. Will it now be contended that the strain upon the cylinder would be increased if the disk were removed, and the office it performs (of preserving the shape of the ring) be effected by the equal radial pressures of the steam?

Such are the conditions of the question as it presents itself to my mind; and they seem to me to consist with the established mode of computing the strength of cylindrical vessels, and with that only. As the elementary facts and principles on which such questions depend are often, however, far from being so obvious as to make the same impressions upon every mind, but, on the contrary, are frequently of so subtle and recondite a character as to elude the grasp of the inquirer, and are perhaps so in this instance, I would not dogmatically aver that the preceding exposition of the subject is sound beyond the possibility of a flaw. I would be glad that the matter should meet the consideration of others competent to its investigation. The question is a most important one, and has not, perhaps, been sufficiently, discussed; at least, I do not at this moment recollect any elementary work in which it is treated of, except the brief notice of it by Professor Barlow, above quoted. It is time it should be finally settled by all the arguments and authorities which may be brought to bear upon it. If the rule heretofore used has indeed given results so much too favourable to the strength of steam boilers as Mr. Bakewell supposes, then the risks from which a watchful Providence has so often preserved those who have to do with them, have been far greater than we have been aware of; and while we feel increased thankfulness for past protection from these perils, we should not be unmindful to guard against them more effectually hereafter.

I have above considered only the effects, upon a hollow cylinder, of the *radial* stress of the confined fluid resolved into two resultants perpendicular to, and acting at the ends of, the diameter. The longitudinal strain parallel to the axis of the vessel has not been taken into view, for it is not clear that it, in fact, augments the tensile effect of the radial strain. It is not necessary, however, to discuss this question at present, and I therefore waive it.

In conclusion, permit me gentlemen, (in reference to the catastrophe which led to this discussion,) to express the pleasure with which I perused, in your last number, the corrected calculations of the stress and strength of the boiler of the Medora, contained in an article prepared from particulars lately furnished by Mr. Reeder, who rebuilt the boiler after its explosion, and whose opportunities of acquaintance with all the details of its structure were much more perfect than my own, and were judiciously availed of by him, and their results lucidly presented by yourselves.—*Franklin Journal*.

Baltimore, May 18th, 1843.

Description of a Reflecting Lantern and a Heliotrope, used by Major J. D. GRAHAM, as meridian marks for great distances, in 1841, while tracing, in his capacity of U. S. Commissioner, the due north line from the monument at the source of the river St. Croix.

THE lantern was constructed by Messrs. Henry N. Hooper & Co., of Boston, under Major G.'s directions. It was similar in form to the Parabolic Reflector Lantern, sometimes used in lighthouses, but much smaller, so as to be portable.

The burner was of the argand character, with a cylindrical wick, whose transverse section was half an inch in diameter, supplied with oil in the ordinary manner. This was placed in the focus of a parabolic reflector, or paraboloid, of sheet copper, lined inside with silver about one-twentieth of an inch in thickness, polished very smooth and bright. The dimensions were as follows:—

	Inches
Diameter of the base of frustrum of reflector, ...	16
Distance of vertex from base,.....	3.75
Distance of focus from vertex, ...	2.25
Diameter of cylindrical burner,.....	.50
Diameter of a larger burner, which was never used, but which,	

by an adapting piece, could be easily substituted, ... 1.25

The instrument answered the purpose for which it was intended, admirably well, and was of great use in tracing the due north line. While it occupied the station at Park's Hill, 15 feet above the surface of the ground, or 828 feet above the sea, in the latter part of September, and early part of October, 1841, the light from it was distinctly seen with the naked eye, at night, when the weather was clear, from Blue Hill whose summit, where crossed by the meridian line, is 1071 feet above the sea; the intervening country averaging about 500 feet above the sea, and the stations being 36 miles apart.

The light appeared to the naked eye, at that distance, as bright, and of about the same magnitude, as the planet Venus. Viewed through the transit telescope, of 43 inches focal length, it presented a luminous disk, of about thirty seconds of arc in diameter. From its brilliancy at that distance, Major G. has no doubt that it would have been visible to the naked eye at 50 miles, and through the telescope at 100 miles, could stations, free from interposing objects, have been found so far apart.

It was remarked, that the wick employed by Major G. was considerably smaller than that usually made, even for parlor lamps; and to this cause he attributed, in a great measure, the perfection with which the parallel rays were transmitted from the reflecting parabolic surface, so as to make them visible at so great a distance. Though a greater quantity of light is generated by a larger wick, the portion of rays reflected in a direction parallel to the axis, and which alone come to the eye, is the smaller as the flame transcends the focal limit. The size of wick most advantageous for use, may easily be determined by experiment: Major G.'s impression is, that, the smaller its transverse section, provided it is only large enough to escape being choked up by the charred particles, even one-third, or perhaps one-fourth of an inch, the farther the light would be visible.

It has occurred to Major G. that lanterns of this description might be used with great advantage as station marks, in extensive trigonometrical surveys, requiring primary triangles of great length of sides. A revolving motion might be given to the lanterns, so as to make the light transmitted from them visible from many different stations within short intervals of time. Their simplicity, and the ease with which they are managed, would perhaps give them, for such purposes, a great advantage over the Drummond, or Bude, lights, even though they be not so brilliant as the latter.

The heliotrope, which he employed in the day time, was made by order of Mr. Hassler, at the instrument shop of the coast survey office. It was a rectangular parallelogram of good German plate glass, $1\frac{4}{5}$ by $1\frac{1}{2}$ inch in size, giving an area of reflecting surface of $2\frac{16}{25}$ square in. This also was seen at the distance of 36 miles.—*The Jour. of the Frank. In.*

Notice of the Discovery of a new Metal by Mosander.

At the meeting of the Scandinavian Naturalists, at Stockholm, Mosander made a communication upon the subject of cerium and lanthanum. He exhibited, in the first place, some pale, clear, lemon yellow oxide of cerium—described, generally, the characters of the protoxide and peroxide of cerium, and showed some of their salts. He then showed the colorless oxide of lanthanum, which remains colorless after ignition, and whose salts are also colorless. He then stated that the ores of yttrium, cerium and lanthanum, are accompanied, in the mineral kingdom, by a twin-brother, which is the

cause of the known rose-red color of their salts, and which it is very difficult to separate from them in consequence of their having the same solvents and precipitants. This body is the oxide of a hitherto unknown metal, to which Mosander has given the name didymium (from the Greek *δίδυμος*, a twin.)

The oxide of this metal is the cause of the brown color of the oxide of cerium, and of the brick-red of the oxide of lanthanum.

He also showed the dark brown oxide of didymium, and, for the sake of comparison, some crystalized salts of cerium, lanthanum, and didymium. The last are red.

He went on to say that he had not as yet succeeded in discovering a method for the perfect separation of these metals, and that this was the cause of the long delay of his communication on the subject of lanthanum; and that, had it not been for peculiar circumstances, he would not even yet have presented the results of his labours, which he could not consider but as imperfect. The preparations, he remarked, were not yet absolutely pure, but they were as nearly so as they could be made by spontaneous crystalizations of large masses of the solutions, in which the oxides had previously been purified as far as possible. A determination of the atomic weight of this metal, on account of the impurity of the preparations, he considered at present useless.—*The Journ. of the Frank. In.*

Method of distinguishing Zinc from Manganese, in Solutions containing Ammoniacal Salts. By M. OTTO.

If solutions of chloride of zinc and chloride of manganese, containing much hydrochlorate of ammonia, be rendered alkaline by solution of ammonia, the addition of the smallest quantity of solution of hydrosulphuric acid precipitates white hydrated sulphuret of zinc, whilst no effect is produced by it in the solution of manganese, more being required to obtain a precipitate of the sulphuret of the latter metal. If acetic acid be then added to the solutions, the sulphuret of manganese dissolves very readily, whilst that of zinc remains undissolved. M. Otto advises the use of hydrosulphuric acid, and not hydrosulphate of ammonia, because the latter, always containing persulphuret may occasion mistakes, since acetic acid separates sulphur from it. If, for example, it be required to determine whether iron filings contain brass, they are to be dissolved in aqua regia, the peroxide of iron is to be precipitated by ammonia, the liquor is then to be acidulated, the copper precipitated by hydrosulphuric acid, and ammonia is then to be added to the filtered liquor, which usually still contains a sufficient quantity of hydrosulphuric acid. If a white precipitate be formed, which does not dissolve in acetic acid, it shows that zinc is present. M. Wackenroder has especially recommended the solubility of sulphuret of manganese in acetic acid, to separate manganese from other metals.—*Jour. de Pharm. et de Chem.*

Thermometric Experiments upon the Light of the late Comet, and upon the Heat of the Zodiacal Light. By A. MATTHIESSEN, of Altona.(Translated from Poggendorff's *Annalen*, vol. 59, p. 171, by Mr. W. LETTSOM.)

SUPPOSING radiation to be the only way in which heat is propagated to great distances, the new comet does not transmit any sensible heat to the earth.

On Monday, the 27th of March, of the present year, at eight o'clock in the evening, a well polished concave mirror, in whose focus a very sensitive air-thermometer was fixed, indicated no elevation of temperature. Whereas there was an elevation observed on directing the axis of the mirror towards the zodiacal light.

The following evening I placed a very delicate thermo-electric pile, constructed by Ruhmkopf, and consisting of 25 pairs, in a little hollow of the ground between the Arc de l'Etoile and the Bois de Boulogne, at Paris, in such a manner that, with the exception of a little grass to the distance of 200 to 300 yards, and a small white house about 800 yards off, with one single window frame to the north-east, it was not within the range of any terrestrial object.

The needle of the galvanometer remained at zero, on the pile, with its conical reflector adapted to it, being directed to the pole-star. The same was the case also when the thermo-pile, provided as before with its conical reflector, was directed towards the tail of the comet, which then stood below Orion. When turned in the direction of the nucleus, the instrument indicated 2 degrees. This indication of heat, however, kept on increasing gradually as the thermo-pile, now beyond the nucleus of the comet, was directed towards the zodiacal light. Below the Pleiades the deflection was 10° , towards the base of the zodiacal light it amounted to 12° , above the point where the sun had set the deflection was 5° . At 9 o'clock, for the comet, the result was the same as before; below the Pleiades, 8° ; at the base of the zodiacal light, 12° ; above the spot where the sun had set, 3° . At half-past 9 the indications were respectively 7° , 10° , and 2° , the result with respect to the comet being as before.

In order to judge of the sensibility of the instrument, it may be sufficient to state that my hand, which from its lying on the moist grass was tolerably cold, drove the needle 90° , against the stop, at the distance of a mètre (39·37 inches English). Without the reflector, the hand produced a similar effect when held at the distance of 25 centimètres (9·84 inches) from the aperture of the pile. The small house, which had been warmed by the sun's rays previous to its setting, brought the needle, at 8 o'clock, to 26° ; at half-past 8, to 21° . At that hour the candle which was burning in the window was extinguished, and the needle receded to 19° . At 9 o'clock the deflection was 13° , and at half past 9, 9° .

With the exception of frequent disturbances of the needle, due to currents of warm air, sufficiently so to be, in many instances, sensible to the feelings, these results, which were repeated more than 40 times, remained constant.

I was surprised to observe that the needle remained unaffected when the instrument was directed to other regions of the heavens, for I expected that in those directions where the stratum of the atmosphere is most dense, or where the sky is more thickly studded with stars, some indications of heat would be given. And I also looked for similar effects from the warmth of the grass, heated in the day by the sun. It may be seen from this how little heat elastic fluids emit, and on the other hand how completely and with what rapidity the grass is cooled down by the dampness of the evening.

As the indications of heat in the direction of the zodiacal light were constant, it remained to be ascertained whether this heat really was caused thereby, or whether it was owing to the atmosphere being warmer in the direction of the setting sun,—the heat emitted from distant objects could not affect the instrument, the latter not being, as before observed, within the range of any such objects. In the former hypothesis the zone of the zodiacal light must possess a high temperature, considering its extreme state of tenuity.

On removing the reflector from the pile, the base of the zodiacal light indicated a deflection of from only 2 to 3 degrees. A single degree to the right or left, zero with regard to the comet.

The colourless flint glass of very high refractive power which was employed in these experiments, and which is made by Bontemps expressly for achromatic microscope-lenses, transmits, at moderate thicknesses, more than three quarters of the rays of heat proceeding from sources at an elevated temperature, whereas it arrests almost the whole of the rays proceeding from a source below the temperature of boiling water.

My lens is 56 centimètres (22·04 inches) in diameter, with a focal length of 16 centimètres (6·29 inches.) When placed facing the base of the zodiacal light it increased the deflection of the needle; it remained steady at 4°. Below the Pleiades the needle went back to 2°, and remained steady. Above the point where the sun had set it receded to zero.

This result arose in part from the following cause, namely, that nearly the whole of the zodiacal light might be concentrated upon the thermo-pile; whereas the space to the right or left is too extended to bring about a sensible increase. But it is also obvious that the increase of heat would not be sensible through the flint-glass unless the source whence it emanated possessed a high temperature. The 5 degrees of deflection in the experiment with the reflector would be principally owing to the warmth of the atmosphere, and be consequently arrested by the flint-glass, whereas the deflection of 15° in the direction of the zodiacal light, proceeded mainly from that phenomenon.

When provided with its reflector this thermo-pile causes a deflection of the needle amounting to 15° on placing a lighted tallow candle at the distance of about 33 feet (English) from it. This shows how insignificant the quantity of heat is which the zodiacal light emits, and that the influence exerted by the comet upon our temperature must in point of fact be inappreciable.

On the New Mode of detecting Arsenic. By Dr. CHRISTISON.

A SHORT time since, Professor Reinsch proposed an entirely new method of detecting arsenic, which consists in acidulating any suspected fluid with hydrochloric acid, and heating a thin plate of bright copper in it, upon which the arsenic is deposited in a thin metallic crust, and then separating the arsenic from the copper, in a state of oxide, by subjecting the copper to a low red heat in a glass tube. Organic fluids and solids may be prepared for this process, by boiling them for half an hour with a little hydrochloric acid, solid matters being cut into small shreds, sufficient water being added to let the ebullition go on quietly. Continue the boiling until the solids are dissolved, or reduced to minute division. Nothing can be more easy than the method of Reinsch. It is also exceedingly delicate; for it will detect a 250,000th part of arsenic in a fluid, and it does not leave any arsenic in the subject of analysis which can be detected by any other means, even by the delicate process of Mr. Marsh. I have lately employed it as the means of furnishing irrefragible evidence in criminal inquiries.

The separation of arsenic upon copper, from a solution, by means of hydrochloric acid and heat, is a new fact in chemistry; and the experiment furnishes a test so far, that if the copper be not tarnished, arsenic cannot be present. But Reinsch's discovery cannot be regarded as a *positive* test, because, as he himself has pointed out, bismuth, tin, zinc, and above all, antimony, will, under the same circumstances, yield a coating to copper sufficiently similar to render it necessary that the deposit be examined otherwise than by the eye only. Reinsch's process, however, is of far greater value than if it merely presented a new test for arsenic. It constitutes the easiest and most secure mode of so separating arsenic from complex mixture, as to enable experimentalists to apply to the metal any of the tests for arsenic already known; and, in my opinion, no method of testing for it approaches the following in conclusiveness:—Cut the copper on which the arsenic is deposited, into small chips, so that they may be easily packed at the bottom of a small glass tube. Apply a low red heat. A white crystalline powder sublimes, in which, in the sunshine, or with a candle near it, a magnifier of five powers will show the equilateral triangles composing the facets of the octahedral crystals, which are formed by arsenious acid when it sublimes. Sometimes the three equal angles, composing a corner of the octahedron,

may be seen by turning the glass in various directions. If triangular facets cannot be distinguished, owing to the minuteness of the crystals, then shake out the copper chips, close the tube with the finger, and heat the sublimed powder over a very minute spirit-lamp flame, chasing it up and down the tube until crystals of adequate size are formed. Next boil a little distilled water in the tube over the part where the crystalline powder is collected, and, when the solution is cold, divide it into three parts, to be tested with ammoniacal nitrate of silver, ammoniacal sulphate of copper, and sulphuretted hydrogen, either in the state of gas or dissolved in water. I am surprised, that during the last four or five years, neither Orfila, nor M. LASSIGNE, nor Liebig, nor Mr. L. Thomson, nor Mr. Watson, nor Mr. Marsh himself, nor any other experimentalist, excepting in Scotland, has thought of applying, as a test of an arsenical crust, the conclusive process described above, and first suggested to me, in 1826, by the late Dr. Turner, which consists in converting the metal into the oxide in such a way as to allow the form of its crystals to be determined. The method has been in constant use in medico-legal researches in Scotland. Yet, what other method is so satisfactory? What substance, other than arsenic, yields the white sublimate with triangular facets, or leaves the substance in such subjection to so many excellent tests?

In boiling substances in the weak hydrochloric acid, a decided excess of acid must always be present—two fluid drachms to every eight ounces of liquid; but if the matter be animated texture in decay, much more acid may be necessary, owing to the presence of ammonia, which gradually neutralizes the acid as the solution goes on. Filtration of the fluid after the acid has acted sufficiently, seems advisable, otherwise organic particles may attach themselves to the copper, and give rise to empyreuma, when the metallic arsenic is driven off by heat. When the arsenic in the fluid is supposed to be small, nearly half an hour should elapse before the copper or copper-leaf is removed. Before applying the sulphuretted hydrogen as a test to the solution of the sublimed oxide, the solution must be acidulated with hydrochloric or acetic acid. In every case the whole process should be applied in the first instance to distilled water, acidulated with the hydrochloric acid to be employed afterwards; and if the copper be tarnished, a purer acid must be obtained, or the copper must be subjected to the subsequent steps of the process, in order to ascertain whether the tarnishing be occasioned by arsenic or not.

I have successfully employed the preceding method in two medico-legal cases, where the bodies had been buried for four months, and I consider that it must soon supersede the beautiful but much more elaborate method of Marsh.—*London and Edin. Jour. of Med. Science*, Sept. 1843.

Examination of the Mother of Vinegar. By MULDER.

THIS investigation was undertaken with a view of tracing the formation of organic substances from inorganic chemical combinations.

Mother of vinegar (*mater aceti*) is a mould-plant which is developed in vinegar, and forms therein a thick leather-like coat similar to the inflammatory crust which covers the crassamentum of blood drawn from rheumatic patients. It is produced not merely in, but from the vinegar, and as it forms the acetic acid diminishes, until ultimately water alone remains.

The mould-plant belongs to the genus *mycoderma* of Persoon, or *hygrocrocis* of Agardh. It is one of the simplest vegetable formations, and belongs to the Fungi rather than to the Algæ. It is formed in vinegar obtained both from wine and beer, but not in that procured from wood. It exists in unmixed vinegar, and also in vinegar in which organic substances are preserved. These substances, however, contribute nothing towards the development of the plant, but merely promote the production of a germ or a cell from which the mould-plant is formed out of the elements of the acetic acid. In all cases, whether organic substances be or be not contained in the vinegar, the mycoderma has the same conformation and chemical composition.

The principal constituents of vinegar are acetic acid ($C_4 H_3 O_3$) and water. In addition to these it contains some salts, a little sugar, gum, extractive, and, above all, some proteine, derived from the albumen of the grape, and dissolved in the acetic acid. The presence of vegetable substances, as cucumbers, cherries, &c., may augment the quantity of proteine, but this increase is not necessary for the formation of the plant.

The nutritious elements of this mould-plant are $C_4 H_3 O_3$ (*acetic acid*.) and $C_{40} H_{31} N_5 O_{12}$ (*proteine*.)

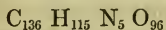
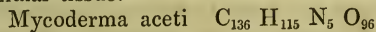
We find these constituents grouped, in a simple form, in the plant, while the proteine and the acetic acid have disappeared from the liquid. The plant contains nothing more; and the transformation of the acetic acid and proteine into a plant can, therefore, be chemically traced.

It is remarkable that the plant always has the same chemical composition, consequently its organization requires a certain proportion of acetic acid and proteine: the latter remains unchanged, but the former takes up water, and becomes cellular tissue. The new product of acetic acid combines in certain atomic proportions with the proteine. This organization demands, therefore, certain chemical proportions just as much as in the formation of sulphate of lime from lime and sulphuric acid.

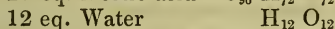
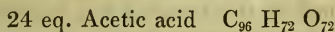
Mulder analyzed three plants formed in vinegars containing different vegetable substances. His results gave as the formula for the plants $C_{136} H_{115} N_5 O_{96}$. The quantity of nitrogen contained in proteine, is taken as the basis of the formula.. By potash all the pro-

teine may be removed, and the residue is pure cellular tissue. The latter, according to Payen's analysis, and corroborated by experiment, has for its formula, $C_{24} H_{21} O_{21}$.

Mycoderma aceti or mother of vinegar consists, therefore, of proteine and cellular tissue.



It has been observed that the proteine is present in vinegar, as albumen of grape. The question now is, in which way $C_{96} H_{84} O_{84}$, or four equivalents of cellular tissue, could combine with one equivalent of proteine, and whence the chief constituent of the plant is derived? The facts that pure vinegar yields the mother of vinegar, and that acetic acid invariably disappears, prove that this cellular tissue is generated from the acetic acid, that is from $C_4 H_3 O_3$. This can take place in no other manner than according to the following plan:—



equal to four equivalents of cellulose.—*Annalen der Chemie und Pharmacie*, Band xlv. Heft 2. May, 1843.

Cement Marble.

An interesting paper on the cement marble of Mr. Keene, was recently read before the London Society of Arts.

This marble is a combination of sulphate of lime (gypsum) with alum. The gypsum undergoes the same preparation as the ordinary plaster of Paris, being deprived of its water of crystalization by heating it in a furnace. It is afterwards tempered with a saturated solution of alum, and when this composition is again calcined and reduced to powder, it is ready for use. The cement is used like stucco; but, as it is susceptible of a high degree of polish, and may be colored by simply dissolving the proper mineral colours in the same water with which the powdered cement is mixed for the workman, we may thus obtain five imitations of mosaics, and of veined marbles. This cement is not applicable to situations exposed to the influence of the weather, or of water; but it has been used in stucco for the interior of Windsor, Buckingham, and other, palaces. As it is of great hardness, it is well adapted to use as a facing instead of tiles; and it makes a good marquetry for paving. The interior of a church in London, and many other public buildings, have been paved in this manner.—*The Journ. of the Frank. In.*

On the Composition of Ferric Acid. By J. DENHAM SMITH, Esq.

(Continued from page 208.)

A second experiment upon a portion of this salt, prepared at a subsequent period to that used in the first experiment, and adapting a tube containing chloride of calcium to the flask, gave from 47·47 grs. of the compound, 4·31 grs. of oxygen, and, exclusive of ash of filters, 0·48 grs. silica, &c., 37·82 grs. sulphate of barytes, and 15·27 grs. of sesquioxide of iron, leaving 2·63 grs. for water and loss.

A third experiment gave 3·06 grs. oxygen, ·69 grs. silica, &c., 27·163 grs. sulphate of barytes, and 10·86 of oxide of iron from 34·47 grs. of the red barytic salt.

To obtain the water, I merely heated the ferrate gradually and gently on a sand heat till it assumed a greenish colour, rejecting those experiments which were partially converted into a light drab colour, which is an evidence of loss of oxygen. 64·48 grs. of ferrate lost 4·93 grs., and 44·41 lost 2·91 grs.; by ignition this green residue was converted into a drab-coloured powder, apparently with the loss of half an equivalent of oxygen, which residue, treated with dilute nitric acid, evolved oxygen gas. Now, estimating the loss sustained in these experiments as water and taking the mean, ferrate of barytes when pure will contain 7·2 per cent. of water, and the three analyses of this barytic salt should respectively indicate, if the loss sustained in these be only water, 2·18 grs., 3·38 grs., and 2·44 grs., a result, as will be seen by reference, so near the loss sustained in analysis, that the difference may fairly be reckoned as merely an error of experiment. The results obtained from the three foregoing analyses, reckoning the loss as water and deducting the silica and sulphate of barytes as impurities, will give respectively 30·38 grs., 46·99 grs., and 33·78 grs., as the quantities of pure ferrate barytes operated upon, yielding—

	I.	II.	III.
Barytes.....	16·14	24·78	17·75
Sesquioxide iron	8·88	15·27	10·77
Oxygen	3·27	4·31	3·06
Water and loss.....	2·09	2·63	2·20
	30·38	46·99	33·78

The mean of which will give 19·56 of barytes, 11·64 of sesquioxide of iron, 3·35 of oxygen, and 2·31 of water contained in 37·06 of ferrate barytes.

The 11·64 parts of sesquioxide of iron are composed of 8·15 of iron and 3·49 of oxygen, which uniting with the 3·55 of oxygen gas (within ·06 of a grain in 37·06 grs. of the quantity theoretically required), will yield 15·19 of teroxide of iron. or ferric acid, combined with 19·56 parts of barytes, indicating the formula $\text{BaO FO}_3 \text{HO}$ as the composition of this salt.

	Exp.	Theory.
1 equiv. barytes	19·56	20·60
1 equiv. ferric acid	15·19	14·06
1 equiv. water	2·31	2·4
	37·06	37·06

I therefore consider the acid oxide of iron present in the deep amethystine solution of the potash salt, and in combination with barytes in the crimson-red barytic salt, to be a teroxide of iron, containing twice the quantity of oxygen existing in the sesquioxide. Whether the pink salt obtained by the agency of chlorine is another and distinct combination of iron with oxygen, forming a still more highly oxygenated compound than ferric acid, I am at present unable to state, but I am inclined to this opinion; it is certain, however, that another acid oxide of iron exists, probably of a lower degree of oxidation than the ferric acid. The combination of this acid with potash or soda is of a beautiful emerald-green colour, precisely similar to the green manganate of potash. The conditions requisite to procure this compound I am at present unable to point out, but I have generally succeeded when I have employed one-half the quantity of nitrate of potash, or what I am inclined to think superior, nitrate of soda, employed to prepare the amethystine salt, and exposed the mixture to a somewhat higher temperature. I have never obtained it unmixed with the purple salt, but owing to its being a much more stable compound than that is, it remains for a considerable length of time undecomposed at ordinary and even elevated temperatures, if excluded from the air. This furnishes an easy method to prove the green salt free from the amethystine salt. Its properties appear analogous to those of the amethystine salts, except with respect to its permanency; acids liberate oxygen, changing the solution to pink, which gradually disappears; chlorine rapidly converts it into the pink salt. It is capable of filtration without being entirely decomposed, but long contact with organic matters appears to resolve it into oxygen and sesquioxide of iron. I regret that at present I have not been able to obtain this green acid of iron in such a state of combination that I can accurately determine its position, but I hope during the next session of the Chemical Society to lay before its members a more detailed investigation of the pink chlorinated compound, and also of the green oxide, and to ascertain their composition and properties.—*Phil. Mag.*

Romford.

On the Bebeeru Tree of British Guiana.

(Condensed from a Paper, by D. MacLagan, M.D., F.R.S.E., published in the Transactions of the Royal Society of Edinburgh.)

“THE bark occurs in large flat pieces, from one to two feet long, and varying in breadth from two to six inches. It is about four lines

thick; heavy, and with a rough fibrous fracture; dark cinnamon-brown, and rather smooth within; and covered externally by a splintering greyish-brown epidermis. It has little or no pungency or acrimony, but a strong persistent, bitter taste, with considerable astringency.

"The fruit is a nut, of an obovate form, slightly compressed. The pericarp is greyish-brown; hard and brittle, half a line thick, rather rough externally, except at the part surrounding the point of its attachment to the footstalk, where it is smooth, and has probably been embedded in the calyx. The cotyledons are plano-convex, and when in opposition, are of the size and general figure of a walnut.

"The precise nature of the tree is unknown; but it is supposed to be a lauraceous plant of large size, the timber of which is known to wood-merchants by the name of *greenheart*.

"Mr. Rodie, the original discoverer, subjected the bark to chemical experiment, and prepared a solution of the sulphate of its alkali. Samples were also sent to manufacturing chemists in London, who operated upon the bark as well as the seeds. By a process similar to that of the Pharmacopœia for obtaining quinine, an alkaline pulverulent uncrystallizable substance was thrown down, most of which is soluble in anhydrous ether, and the remainder in alcohol. The ethereal solution being treated with animal charcoal, filtered and evaporated, yields shining reddish-brown scales, not crystalline. The substance, which is called *Bebeerine*, is found to have antiperiodic properties, having been administered with marked success in agues and intermittent fevers. The alcohol solution above mentioned yields an alkaline matter, which has been called *Sipeerine*, but which has not been minutely examined.

"The tannin of *bebeeru bark* resembles, in its general characters, that variety which exists in the cinchona barks."

This tree is not at present in the market as an article of the *Materia Medica*; but the author of the paper states that he has received a barrel of the bark, and likewise of the fruit of the plant. If repeated experiments should confirm the opinion already expressed respecting its utility, a further supply might be obtained from British Guiana, where the tree is indigenous.—*Phar. Jour.*

Simple Method of ascertaining the presence of Minute Quantities of Nitrogen in Organic Substances. By M. LASSAIGNE.

THE presence of nitrogen in organic substances is generally indicated by different reactions, which isolate this body, in the gaseous state, by burning the combustible elements with which it was combined, or which transform it into ammonia, the properties of which are easily distinguished.

These methods, which are in daily use among chemists, answer very well for determining the proportion of nitrogen in a weighed quantity of organic matter, but are almost inapplicable when the quantity of matter to be examined is very minute.

The process which I have adopted, after submitting it to numerous trials, is so sensible as to afford indications of the presence of nitrogen in substances containing that body, the quantities of which could scarcely be estimated by the most delicate balance. The principle upon which this process is founded, although recognized by chemists, has not heretofore, that I am aware, been applied to this purpose. The method alluded to depends upon the facility with which cyanide of potassium is formed when an excess of potassium is calcined to dull redness, in a tube, with an organic substance containing even a small proportion of nitrogen. The product of this calcination being mixed with a few drops of cold distilled water, affords an alkaline liquor, which, mixed with a soluble ferroso-ferric salt, occasions a greenish or yellowish blue precipitate, which, in contact with a few drops of hydrochloric acid, is rendered perfectly blue.

Quantities inappreciable by Fontin's balance, of *urca*, *uric acid*, *allantoine*, *albumen*, *fibrin*, *dried gluten*, *morphia*, *narcotine*, or *cinchonia*, calcined in a small glass tube, after placing them on small fragments of potassium, have always afforded distinct, and well-defined reactions, in proportion to the quantities of nitrogen contained in these organic substances. Comparative experiments made on non-azotized substances, such as *pure sugar*, *starch*, *gum*, and *salicine*, afforded no similar reaction. Lastly, in operating on complex substances, containing azotized and non-azotized principles combined, the presence of the nitrogen has been distinctly indicated in extremely small quantities of the substance. Thus, in a few minutes, the nitrogen may be detected in a small portion of wheat, of barley, or of dried crust of bread.

The minute quantities of matter on which we operate by this process renders it necessary to use a very small glass tube, about one inch in length and the twentieth of an inch in diameter. A small fragment of potassium, about the size of a millet seed is to be introduced into this tube, previously sealed at one end. The potassium being moulded into a cup shape with a piece of platinum wire, the substance to be calcined is to be introduced into the hollow part. In some cases, where the matter operated upon is volatile, it should be placed under the potassium, so that it may be brought in contact with the metal during the action of the heat. These arrangements being made, hold the tube near to its open end by means of a forceps, and heat it gradually in the flame of a spirit lamp, until the excess of potassium is driven off in vapour through the carbonized organic matter. This point is easily known by observing the greenish vapour formed by the potassium at a little distance from the heated part of the tube. After having raised the heat to dull redness, the tube is to be withdrawn from the flame and allowed to cool. To obtain the product of the calcination, the little tube is to be cut in two by means of a small file, and then put into a porcelain capsule, and four or five drops of distilled water added to dissolve the cyanide which may have been formed. The resulting liquor,

decanted from the carbonaceous residue, is to be tested with a drop of ferroso-ferric sulphate (sulphate of magnetic oxide of iron), when a dirty green precipitate will be immediately formed, turning to a fine blue on the addition of a drop of hydrochloric acid, if the substance operated upon contain azote, even in minute quantity. If no azote were present, the hydrated oxide of iron, occasioned by the addition of the salt of iron, will be entirely redissolved without causing any blue colour.

The formation of cyanide of potassium, resulting as is known, from the calcination, under suitable circumstances, of azotized organic matters with potash, might lead to the expectation that if the potassium were replaced by hydrate or carbonate of potash, in the process above described, the result would be similar; experience, however, has proved the contrary, at least in those cases in which I have experimented. Thus, equal quantities of azotized organic matter, calcined to dull redness in small tubes with caustic potash and with carbonate of potash afforded no cyanide of potassium, while the calcination at the same temperature with potassium, always afforded a product the reaction of which clearly indicated the presence of cyanide.

The use of potassium, then offers an evident advantage in the cases alluded to, for determining the presence of minute quantities of azote. The result, however, may be considered as doubtful, if a non-azotized substance might be accidentally mixed with a small quantity of some nitrate or ammoniacal salt. In these two cases, the potassium, acting on such salts, together with the carbon of a non-azotized body, may produce a little cyanide of potassium, which would afterwards afford the characteristic reaction with the salt of iron. The experiments I have made with starch mixed with a little nitrate of potash or ammoniacal salt, have shown that under these circumstances a very small quantity of cyanide of potassium is formed,

The examination of an organic body for azote, as a constituent principle, should generally be made on substances previously purified, and, consequently, deprived of nitrates and ammoniacal salts, the presence of which is so easily ascertained by means already known, and, with this precaution, the process here described is applicable in most cases. This simple method should especially be adopted when the object is simply to obtain clear evidence of the presence of azote.

Many applications of this process, so easy of execution in a few minutes, will no doubt suggest themselves. I have already employed it for determining the presence of azotized matter in the excrements of a mouse which had been fed on *pure starch* and *pure sugar*. This observation, which, at first sight, appears inconsistent with the chemico-physiological views recently promulgated, confirms those put forth in 1825, by M. Leuret and myself—that *alimentary matter, in passing through the intestinal canal, becomes mixed with foreign substances; and that the excrementitious matter, formed after*

the process of digestion, need not consist exclusively of the product of that process, but is more or less mixed with principles furnished by the different secretions of the digestive canal.

The same method of examination, applied to the study of several products resulting from the prolonged action of solution of ammonia on olive oil, has enabled me to detect the formation of an *azotized substance, neutral and crystallizable*, which for the present I have named, from the manner of its production, *elamine*. The first syllable of this word is taken from the Greek word *ελαιον* (oil), and the second from the three initials of the word *ammoniaque*. This action of ammonia on olive oil, which thus differs from that exercised by the other alkalies, appears to me to resemble, in some respects, the reaction which has been remarked by MM. Dumas and Boullay, on treating certain ethers with ammonia. This observation establishes a new comparison between certain fatty substances and some of the ethers.

From the facts and observations above stated, it follows :

1st. That potassium, heated to dull redness with an azotized organic substance, easily transforms it, in part, into cyanide, the presence of which may afterwards be detected by a salt of iron.

2nd. That in this process, under the conditions in which it is employed, to afford a sensible indication of azote in a minute portion of organic matter, the potassium cannot be replaced either by the *hydrate* or the *carbonate of potash*.

3rd. That the application of this simple process to the detection of azote in a variety of products of organization, affords the means of solving numerous important questions in animal and vegetable physiology, where it is necessary to submit the products to very numerous experiments.—*Phar. Jour.*

Chromatype, a new Photographic Process. By Mr. R. HUNT.

WE are indebted to Mr. Ponton for the discovery of the first photographic process in which chromic acid was the active agent. He used a paper saturated with the bichromate of potassa, which, on exposure to sunshine, speedily passed from a fine yellow colour to a dull brown, giving, consequently, a negative picture. M. E. Becquerel improved upon this process, by sizing the paper with starch, applying the bichromate of potassa, which enabled him to convert the negative picture into a positive one by the use of a solution of iodine, which combined with the starch in those parts on which the light had not acted, or only slightly acted, forming the blue iodide of starch. These pictures are, however, tediously produced; they are seldom clear and distinct, and failure too frequently attends the utmost care.

While the author was perusing an extensive series of researches on the influence of the solar rays on the salts of different metals, he discovered a process by which positive photographs are very easily

produced. Several of the chromates may be used in this process ; but Mr. Hunt prefers those of mercury or copper, the most certain effects being produced by the latter chromate, and, indeed, in a much shorter time than with any other of the chromates.

The papers are prepared in the following manner :—

Good writing paper is washed with a solution of the sulphate of copper and partially dried ; it is then washed with a solution of the bichromate of potassa and dried at a short distance from the fire. Papers thus prepared may be kept for any length of time, and are always ready for use. They are not sufficiently sensitive for use in the camera obscura, but they are available for every other purpose. An engraving—botanical specimens or the like—being placed upon the paper in a proper photographic copying frame, it is exposed to sunshine for a time, varying according to the intensity of light, from five to fifteen or twenty minutes. The result is generally a negative picture. This picture is now washed over with a solution of nitrate of silver, which immediately produces a very beautiful deep orange picture upon a light dun coloured, or sometimes perfectly white ground. This picture is quickly fixed by being washed in pure water and dried.

The author remarked that, if saturated solutions were used, a negative picture was first produced, but if the solutions were diluted with three or four times their volume of water, the first action of the sun's rays was to darken the paper, immediately upon which a very rapid bleaching action followed, giving an exceedingly faint positive picture, which was brought out in great delicacy by the nitrate of silver. It is necessary that pure water should be used for the fixing, because the presence of any muriate damages the picture, and hence arises another pleasing variation of the chromatype. If the positive picture be placed in a very weak solution of chloride of sodium, the images slowly fade, leaving a very faint negative outline. If it be taken from the solution of salt and dried, a positive picture of a lilac colour may be produced by a few minutes' exposure to the sunshine. Prismatic analysis has shown that the changes are produced by a class of rays which lie between the least refrangible blue, and the extreme limits of the violet rays of the visible prismatic spectrum—the maximum darkening effect being produced by the mean blue ray, whilst the bleaching effect appears to be produced with the greatest energy by the least refrangible violet rays.—*The Chemist.*

The Smoke Nuisance.

A FEW remarks on this subject appear particularly called for at the present time, when legislative interference is likely soon to be interposed to suppress the nuisance, and while much difference of opinion exists among manufacturers as to the best means to be adopted for preventing the emission of smoke from their factory chimneys.

For upwards of 20 years many plans have from time to time been suggested for the *combustion* or *prevention* of smoke, but with what success we are left to judge from the fact that none of them singly, or any combination of them, has been generally adopted. What we therefore propose is to examine into the probable cause of this extensive failure of the numerous ingenious plans proposed for "smoke burning," and to suggest what appears to us the only sure method of effectually, and economically, securing the great benefit to be derived from an improved method of constructing furnaces to secure the perfect combustion of the fuel, whether of its solid portion, or of its aëriform or gaseous products.

All the plans devised for burning coal without the emission of smoke from the chimney top, are, 1st. dependent on mechanical arrangements, without any consideration for the nature of the fuel, or its resulting products; or, are, 2nd. mechanical arrangements calculated to favor certain well-known chemical conditions.

To the first class belong all those patented furnaces called "smoke burners," for—as Watt describes the process—"passing the smoke *over, through, or among red hot fuel.*" Some of these inventions provide double grates, others revolving grates, some have an endless chain of bars, &c. &c.; another kind takes the smoke from the chimney by means of a fan and blows it into a closed ash-pit, whence it passes through the incandescent fuel on the grate. All these methods literally provide for *burning smoke*, and the patentees have put on record in their specifications their ignorance of the chemistry of combustion, by claiming only to purifying the smoky vapor of its blackness by the action of heat, instead of first ascertaining its origin, and then endeavoring to prevent the very formation of smoke. The "smoke burning furnaces" provide, in fact, for burning the largest quantity of fuel with the smallest quantity of air, than which nothing is more opposed to the simplest of chemical facts, viz., the large quantity of air required by the gaseous products of the coal.

The second division of the methods under consideration provides for the admission of air to the gases of the fuel, whether olefant gas, carburetted hydrogen gas, or carbonic oxide. These methods may be said to have little to do with the furnace itself: the grate, ash-pit, flame bed, flues, &c., remain unaltered, and what is chiefly attended to is the admission of air. These plans may be designated the chemical processes, the others being entirely mechanical, and opposing, rather than favoring, chemical views.

Air may be employed either hot or cold. Many methods have been used to heat the air, as by hollow bars, dead-plates, and by coils of pipe placed in the chimney as used by Mr. Coad, or by a series of vertical pipes—an improvement on Mr. Coad's plan, patented by Mr. Samuel Hall. Two difficulties occur through heating the air: 1st. that of obtaining sufficient air in the furnace; and, 2nd. every measure of heated air contains less oxygen than air of atmospheric temperature; and it being the oxygen alone that we require for promoting the combustion of the gas.

Of the hot air plans we hear of very few being in use, and they only partially answer, where the coal is not bituminous. Mr. J. Parkes, about 20 years back, admitted air through a double bridge, by which it entered in one sheet or stream of the full width of the furnace; but to this method there were many practical objections. Another, and a highly ingenious plan is that of Mr. Charles Wye Williams, manager of the City of Dublin Steam Packet Company. It consists in the direct admission of air to the coal and other gases, in the form of small films or lets. The apparatus for throwing in the air is placed in front, at the sides, at the top of the furnace, or behind the bridge, as circumstances require, the patentee having stated in his claim that he does not confine himself to the particular number, situation, or dimension of the several parts of the air-distributing apparatus. Mr. Williams is the author of an elaborate treatise on the "Combustion of Coal" chemically and practically considered, which manufacturers desirous of obtaining information on this important subject will do well to peruse.

What has been long well known in laboratory experiments, Mr. Williams has very ably and ingeniously applied on the large scale of the furnace. It is a plain and simple fact that every fresh charge of fuel is attended with a sudden and large evolution of crude coal gas requiring air in the proportion of about ten measures of air to one measure of gas, or more correctly for the various gases as follows:—

Bicarburetted hydrogen.....	H ₃	C ₆
Carburetted hydrogen.....	H ₂	C
Carbonic oxide.....	O	C ₂

requiring to a measure of each for the first, 15 measures of air, for the second, 10 measures, and for the third, 5 measures.

Mr. Williams's method is called the "Argand Furnace," and in principle it agrees precisely with the Argand oil in gas lamps. The division of the air into small streams is undoubtedly the best for obtaining the rapid and intimate admixture of the air and gas, and the admission of the air at the common temperature brings more of the atmospherical oxygen in the same space than if the air were heated, particularly when the air is admitted at the ordinary pressure and not impelled by a fan or other blower. If we take, for example, a cubic foot of coal gas, and ten cubic feet of air, it is almost self-evident, that, to get them mixed, we must divide one or both into small streams, otherwise we shall have imperfect combustion and a deposition of carbon, which, mixed with the usual products of combustion, would afford what is popularly called smoke, but which, having no other combustible in it than the carbon, cannot be economically consumed, though it may be scientifically and most advantageously prevented.

The attempts to consume *smoke*, instead of being directed to the conditions requisite for the perfect combustion of the gas have led to many serious and expensive blunders, and as no plan can be lasting which is based on incorrect principles, though it may be tem-

porarily forced into use, and for a while deceive the unsuspecting, we have much pleasure in bearing testimony to the leading facts of the case as respects the abatement of the smoke nuisance, and may safely affirm that unless based on chemical principles no plan can be safely and satisfactorily recommended. The smoke being the result of the imperfect combustion of the impure gas, all we have to do is to provide sufficient air, in the best manner, to obtain its perfect mixture preparatory to its ignition, and by no other method can economical combustion be conducted.—*The Chemist*.

Newly discovered Mine of Manganese, near Giessen.

WE have been informed that manganese of very superior quality has recently been discovered in the neighbourhood of Giessen. An essay made by M. Barruel has been forwarded to us, which very satisfactorily proves its superiority. Professor Liebig always uses it in his laboratory, and speaks very highly of it. It has also been analysed by Dr. Ettling. We expect to receive a sample of it very shortly, and will immediately perform and publish an assay of it, in order that our readers may be enabled to judge of it for themselves, and to avail themselves of the advantages which it offers. We understand that it can be very economically imported to England.—*Ibid*.

New Process for preparing Cyanogen. By MR. ALEX. KEMP.

Take six parts of perfectly dry ferrocyanide of potassium, and nine parts of bichloride of mercury, both in fine powder, and mix them intimately together, then apply heat to the mixture in a glass retort when cyanogen gas will be disengaged; mercury at the same time distils over, and a dark colored matter is left in the retort, being a mixture of chloride of potassium and cyanide of iron.—*Phar. Jour*.

Some Observations on the present State of Geological Inquiry as to the Origin of Coal. By JOHN HAWKSHAW, Esq., F.G.S., M.I.C.E., &c.*

[Read before the Manchester Geological Society, 25th March, 1843.]

WHATEVER might be the case twenty years since, it is not at the present time worth any one's while to aim at being the founder of extensive geological theories. By harder working and greater fitness for the task, one person may be farther in advance than others, and have made larger and more important deductions; still he cannot go much beyond the legitimate conclusions of his inquiry; or, if he does so, it must be with the consciousness that while he *speculates*, another may have proved the *contrary*. Keeping in view this peculiarity of geological research, which is more applicable to it perhaps than any other of the sciences of the present day, there seems no great danger of making too extensive generalizations, or at all events, the mischief resulting from overstepping right philosophical boun-

* Communicated by the Author.

daries will be to the transgressor and not to the public. There is not in the present day we believe, any science, at the head of whose votaries stands a name so lofty, above the rest so low, as that his utterances shall be to them in place of their own investigations. Still we are not among those who believe that great evils are to be ascribed to theorizing. It is natural to men, though seeking after truth, to be led beyond their facts. It is the result of that function of the human mind by which it is impelled forward out of the known into the unknown; and in consequence of which the higher spirits of our times are ever dwelling upon what to many may appear little more than hopeless obscurity.

There would be no progress without this. The darkness of the middle ages consisted in men having ceased to speculate. The efforts of a few minds who had lived centuries before were to them in place of all effort of their own. Had they begun to speculate they would have sought to prove. The peculiarities of the schools themselves should rather be ascribed to their circumstances than to faults of system: they arose out of the strivings of great and acute minds, labouring with inefficient means to unlock the mysteries of nature. These would have been better workmen had they had better tools. To have limited their speculations more within the compass of their real knowledge, was only desirable on the hypothesis that their information admitted of being greatly extended. This may be questioned. Both experiment and observation must await the progress of the arts. The vision of the astronomer extends; but only with the improvement of the telescope—is limited by its imperfections. The inquiries of the geographer cannot reach farther than the progress of navigation. Yet Columbus believed in the existence of another continent before he saw it; and would never have searched for it but for his previous speculation as to its existence. In like manner the telescope of the great Herschel might never have been constructed, nor his brilliant discoveries made, but for previous theories that were not verified by their framers.

The fault of the dark ages then was certainly not that of too much speculation. This was prohibited. Mind was stagnant. "There was no undulation and therefore no light." That which to Grecian sages had been no more than feelings after the unknown, was assumed to be the ultimatum of all knowledge. Not because the spirit of the Grecians lived—it was dead; nor because Aristotle had written—it would have been the same or worse had he never existed—but because freedom of thought was prohibited.

Bacon's writings were the symbol of better times. Men at his day were free to seek knowledge: and they made greater progress than in classical times, because they sought it under better auspices. Still the excellence of all that was either said or done in those days, does not so clearly appear by fair contrast with what had been done 2000 years before, as by comparison with the gloom and the nothingness of the long ages that succeeded to that earliest period.

Both as it respects experiment and observation the present times surpass those, and future times will surpass these ; unless men are again doomed to a state of thralldom. But what we chiefly wish to impress is, that there has, in all probability, been more speculation and theorizing since Bacon lived than before. There can indeed be no extension of the boundaries of knowledge without it. No experiment perhaps, is ever performed, (except by rote or for amusement which then becomes mere matter of imitation) which is not instigated by theory more or less extensive, it may be incipient. The mind expands with every new fact, and as from a higher point extends a wider gaze. In one sense indeed, it may be said to be proceeding *with* discovery ; but the truths ascertained must always be a certain distance behind, not perhaps of the real knowledge of an age, but certainly of those who are increasing and extending that knowledge. And save in cases where men have stumbled upon important discoveries by chance or accident, it is doubtful whether men greatly wanting in power of imagination, which may be considered nearest akin to the creative faculty, can ever be expected greatly to extend the domain of human knowledge.

The application of the preceding observations to what we have yet to say, will be easily made. The last few years of geological inquiry may seem particularly to call for them. It has not unfrequently occurred that some of the leading minds in these pursuits have reversed their former conclusions by their subsequent discoveries ; in other cases their conclusions have been reversed by the discoveries of others. This is to be attributed to the rapid accumulation of geological facts, which is constantly altering and remodeling preconceived opinions. Still there is steady progress. It rarely occurs but that the discoveries which uproot one set of opinions confirm others. Some, by repeated confirmation are established ; others are altered and modified. It must also be remembered that geology is comparatively a young science. More probably has been done for its advancement by the present generation than in all time preceding.

That portion of this science of which we propose to treat is yet new. And we are still deficient in information to enable us to pronounce upon the origin of our coal fields with anything like certainty. It was but lately established that coal generally is of vegetable origin. Professor Johnstone has shown by chemical analysis that all coal is composed of carbon, hydrogen, and oxygen : and that the varieties of coal are caused by different combinations of these elements. The great value of the article itself appears to have obstructed a geological acquaintance with it, by leading coal owners rather to withhold than to impart information respecting it. Moreover the operations of coal miners are necessarily slow when considered in relation to the large extent and great number of the coal seams.

In the coal field of Northumberland, for instance, there are no less

than 32 beds of coal, in the South Wales coal field 23 beds, and in the Lancashire and Cheshire coal fields Mr. Binney enumerates 35, while the various measures of sandstones, shales, &c., forming the carboniferous series have a thickness in some places of 4-5ths of a mile.

In addition to the extensive coal fields of the United States, which present many points of resemblance to those of this country, coal has also lately been met with though in small quantity in South America, 40 or 50 miles to the westward of Porto Cabello. It is also found in the centre and south of France, near the Loire and the Allier, and in the north of France, in the neighbourhood of Boulogne and Valenciennes: also in Begium, in the north of Germany, in the north-east and south-east of the Harz mountains, in Saxony; in Bohemia, in Sweden, in Southern Russia, and in Spain.

Coal of an inferior quality has long been worked in China; it is also found around the Persian Gulph; and in most parts of Cutch. There are also coal seams in the district of Burdwau, 130 miles from Calcutta; near Bhaugulpoor on the Ganges; on the Banks of the Hooghly, and in the Birman empire. It occurs also in Australia and Van Diemen's land. Thus, though found only in detached patches or basins, which are small when compared with the earth's surface, this valuable article is extensively distributed; which considering its vegetable origin was to be expected; as wherever the earth was full of vegetable life, there other circumstances permitting coal might be formed.

No one can carefully examine the vast assemblage of limestones, sandstones, and shales, sometimes laminated, sometimes stratified, and every where, from the floor of old red sandstone to the topmost of the series, quite conformable to each other, without coming to the conclusion that the rocks are of sedimentary origin: and that the period of their deposit was one of quietness and repose—the extensive dislocations and faults of the carboniferous series having plainly occurred subsequently to the formation of the whole.

It was very natural to infer that the beds of coal interleaved among these sedimentary strata were of like origin with them—that the whole were deposits from water; the vegetable matter having been floated to its resting place from higher lands whereon it formerly grew. This, which may be termed the “Drift Theory,” will be explained by the following extract from Dr. Buckland's “Geology and Mineralogy,” vol. 1, p. 46:—“The strata in which these vegetable remains have been collected together in such vast abundance, have been justly designated by the name of carboniferous order or great coal formation. It is in this formation chiefly that the remains of plants of a former world have been preserved and converted into beds of mineral coal; having been trees transported to the bottom of former seas and estuaries or lakes, and buried in beds of sand and mud, which have since been changed into sandstone and shale.”

As a further exemplification of this theory, and also at the same

time as illustrating the character of the coal formation, we will give another extract from the preceding work (see page 64, vol. I.):—
“The most characteristic type that exists in this country of the general condition and circumstances of the strata composing the great carboniferous order, is found in the north of England. It appears from Mr. Forster’s section of the strata from Newcastle-upon-Tyne to Cross Fell in Cumberland, that their united thickness along this line exceeds 4000 feet. This enormous mass is composed of alternating beds of shale or indurated clay, sandstone, limestone, and coal: the coal is most abundant in the upper part of the series, near Newcastle and Durham, and the limestone predominates towards the lower part; the individual strata enumerated by Forster are thirty-two beds of coal, sixty-two of sandstone, seventeen of limestone, one intruding bed of trap, and one hundred and twenty-eight beds of shale and clay. The animal remains hitherto noticed in the limestone beds are almost exclusively marine; hence we infer that these strata were deposited at the bottom of the sea. The fresh water shells that occur occasionally in the upper regions of this great series show that the more recent portions of the coal formations were deposited in water that was either brackish or entirely fresh. It has lately been shown that fresh water deposits occur also occasionally in the lower regions of the carboniferous series (see Dr. Hibbert’s account of the limestone of Burdie House, near Edinburgh, (*Trans. R. S. of Edin.*, vol xiii); and Professor Phillips’s notice of fresh water shells of the genus *unio*, in the lower part of the coal series of Yorkshire; *Lon. Phil. Mag.* Nov. 1832, p. 349). The causes which collected these beds thus piled above each other, and separated by strata of vast thickness, composed of drifted sand and clay, receive illustration from the manner in which drifted timber from the existing forests of America is now accumulated in the estuaries of the Mississippi, and on the river Mackenzie, (see *Lyell’s Prin. of Geol.*, vol. 3, B. 3, chap. xv., and Professor Phillips’ *Art. Geol. Encyclopædia Metrop.* part 37, p. 596).”

This may be taken as sufficiently explanatory of the opinions of most of our older geologists, and which until of late almost wholly prevailed. It was a reasonable and obvious inference, and strong proofs should no doubt be required to bring us round to the conviction that though, it be conceded that sandstones, shales, &c., under and overlying coal seams were deposits from water, we must look to other causes for the existence of the coal seams themselves.

To some geologists it has appeared that there are sufficient grounds to induce them to ascribe the origin of these coal seams thus interposed amongst the sedimentary beds to other and quite different processes. They consider that the plants and vegetables from which seams of mineral coal have resulted grew on the very spot where now in a greatly changed form we find them.

We cannot do better, for the purpose of stating these later views, which for the sake of contrasting with the drift theory we may call

the theory of "local growth," than to give them in the terms in which they were advanced by the late Mr. Bowman, in his article on the "Origin of Coal," (in the Geol. Trans. of Manchester, vol. 1, p. 105), where he thus writes. "Jameson long ago considered that the plants from which coal was formed, once stood and grew in the places where they were buried. Brogniart thought it probable that the beds were formed from vegetables, which after their death passed into a kind of peaty deposits of greater or less extent, on which other vegetables still grew, till as De Luc imagined, they afterwards slipped into the sea, or by other means, became covered by beds of rocks, and there gave rise to coal deposits. If, for this awkward mode of entombment in the waters, we substitute a submergence of the land when covered by the vegetable mass, I think we shall not be far from the truth. The authors of the "Fossil Flora" after adverting to various circumstances, arrive at the conclusion that the beds of coal chiefly originated in vegetable matter which lived, died, and was decomposed upon the spots where we now find it; each bed having been the product of an extended surface of marshy land, covered with a rank luxuriant vegetation. (Pref. to F. F., vol. 2, p. 22). These views have been recently advocated in a paper, read by Mr. T. B. Beaumont before the London Geological Society, and are in unison with my own. My opinion is simply this—That the trees and vegetables from which the beds of coal are derived, grew on the identical spots the latter now occupy, when each bed was successively the surface, and probably was but little raised above the level of the waters, either as detached islands or extensive plains or savannahs; that these surfaces during the settling of the earth's crust, were one after another submerged and covered with sediments from turbid waters, or with drifted clay, sand, and shells which buried up the plants; that these sedimentary deposits *gradually* accumulated till they formed a new surface, which in time produced another growth of plants and trees, and after a second period of rest, were in their turn submerged and covered up by other deposits; and that similar intervals of repose with intermittent occasional subsidences, were repeated during the entire period of the coal formation. As such vegetable surface sunk beneath the waters, it gradually became converted into coal by processes already explained, and the successive deposits of mud and sand became consolidated into shales and sandstones."

"The enormous aggregate depth of the whole coal formation, and the great thickness of some of the beds of shale, &c. incontestibly prove the long periods that must have been required to complete them. This slow deposition is in harmony with what we observe in all other geological epochs, and has kept going on till the accumulated sediment has formed a shoal which has gradually reached the surface, by degrees has resisted the encroachment of the waters, and then established itself as an island. As the channels between different islands become choked up with sediment, marshy plains or

morasses would be formed, and would unite the islands into plains, upon which vegetation would soon commence, and favoured by great heat and moisture, would rapidly prepare a soil for the growth of the larger succulent plants and for forests of hard-wood trees and coniferæ."

In turning from these views of Mr. Bowman to the drift theory, we would observe that if we are to look upon coal as a deposit from water it is obvious that vast regions of vegetation must have been ransacked to form some of our principal coal seams, a single bed in the neighbourhood of Wolverhampton being 10 yards in thickness; and seams 3 yards in thickness being of frequent occurrence. Mr. Bowman in his paper on the "Fossil Trees found near Manchester," (*Geolog. Trans. Man.* vol. 1, p. 139,) endeavours to estimate the thickness of the vegetable mass from which was derived the small seam of coal at the roots of those trees, and arrives at the conclusion that it had lost two-thirds of its original bulk by shrinkage: or in other words, that the vegetable mass requisite to form a coal seam would require to be three times the thickness of the coal seam. Without taking this calculation for more than its worth, it will be readily granted that the supply of vegetable matter to form our principal seams must have been large indeed.

Now we have to consider how this mass of vegetation could be brought to the spot whereon it permanently rested. To those who have been familiar with tropical rivers, it will have occurred that it is during those seasons when swoln by the rains that they carry the largest quantities of vegetable matter to their mouths; but then it is also that they are the most turbid and muddy. It would therefore happen that the vegetable and earthy matter would be deposited together, which could thereafter become nothing more than irregular beds of lignite, "interstratified with the clays and sands, and often irregularly diffused through them" like the lignite of the Eifel mentioned by Lyell (*Princp. Geol.*, vol. 3, chap. 14): or, otherwise (as we can conceive might be the case), the vegetable and the earthy matter might become separated by their different specific gravities and would hence be deposited, in different localities, provided currents existed in the water; or in successive layers if the water was still. We should expect, however, after the former process, to find beds of earthy sediment running into taluses of vegetable; or we should look to find irregular patches of the latter varying in assortment and thickness. It would only be where deposited in perfectly still water that we could expect any uniformity of distribution. And in the latter case we may well suppose, though in the first instance earthy and vegetable matter had been floated together into some lake or inland sea, that the earthy sediment might be first to fall to the bottom, forming what should afterwards be the floor of the coal seam; and that the vegetable mass, after floating about many days until covered with vegetation (as will be instanced hereafter), and fully saturated with water, and its specific gravity thereby increased, might then also

settle quietly to the bottom, covering the previous deposit with sufficient regularity to become in after times a coal seam little varying in thickness. It is true there would still remain the difficulty of accounting for the evenness both at top and bottom of the coal seam thus formed: but we do not know to what extent, ere it became covered up and subjected to pressure by a succeeding deposit, the vegetable layer might not from decay or some bitumenizing process have become softened, flattened, and incorporated into one homogenous mass.

Were the vegetables thus to be converted into coal small in species, as Algæ or ferns and grasses, then the regularity of surface would easily obtain; and it would also be more easy to account for the fine impressions of those minor vegetables found in the roofs of coal seams. And this might, to a great extent, be the case, though here and there trees and plants of larger growth were embodied with them.

It should not be lost sight of that on the theory of local growth, except on the assumption that the fields of vegetation submerged were very analagous to our peat mosses, there seems as great difficulty in accounting for the smoothness of upper surface as on the drift theory. With regard to the successive subsidence of tropical marshes, where the diversity of form and species of the vegetation would be great, it is evident that some levelling process would be necessary on either theory: and probably this in both cases will have to be looked for in some chemical change or bitumenizing process, about which most likely we require to know more, before our conclusions on this difficult subject can be very satisfactory. For unless we conceive some such change to have commenced before the deposit of the overlying stratum of earth, we have to meet the difficulty of a naked deposit of wood, or of smaller vegetables, being immediately overlaid by an earthy stratum of soft mud or sand, and yet the line of separation between the two being a plane and smooth surface: a result we think which under the circumstances would be impossible.

We are not aware that it is essential to, though it frequently accompanies the statement of the drift theory, that the vegetable matter thus floated to a resting place should chiefly consist of timber or trunks of trees: were it so there would still remain the difficulty, after this mass of matter had been reduced to a plane surface by a chemical change, and before the subsidence of the next stratum, of accounting for the fine fossilized tracery of minute vegetables, as ferns, &c., so frequently found at the roofs of coal seams. And whether the vegetables were large or small, on the hypothesis of their being floated, into bays, estuaries, or lakes, we should expect that it would not be the more delicate parts of vegetation that would be preserved, as the points of the fern and not their stems; nor should we look to find trees thus transported, in possession of their bark, as is usually the case with fossil trees of the coal formation.

(To be Continued.)

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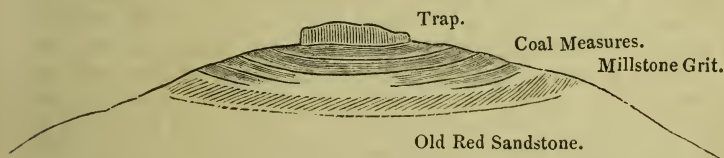
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Some Observations on the present State of Geological Inquiry as to the Origin of Coal. By JOHN HAWKSHAW, Esq., F.G.S., M.I.C.E., &c.

(Continued from our last.)

If we carefully consider the sections of some of our coal fields we are struck with their greater resemblance to lakes that had gradually silted up than to raised estuaries, and hence the term basin which is applied to them. In some instances, as in the coal measures of the Brown Clee Hill, Salop, (described by Mr. Romley Wright, vol. 5, p. 1., Lond. Geol. Trans., second series,) an appearance is presented not unlike the sections given by Mr. Bowman to illustrate



the formation of coal by subsidences. For that theory seems to require that the coal should be formed rather on elevations than in deep basins. Though with respect to the Brown Clee Hill it is a question whether or not the basalt now crowning both summits, and overlying the coal formation, is not an evidence that the latter was pushed up to its present form.

The occurrence of pectens, goniatites, ammonites, and other shells of marine origin, appears to prove that the deposits where they occur were from salt water: while the presence in the same coal field of other shells of fresh water origin, as of the unio, would indicate that

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at other periods fresh waters prevailed. Professor Phillips in his article on Geology in the *Encyclopædia Metropolitana* mentions two layers of these fresh water shells as lying in the midst of the marine beds; though in the coal series of Northumberland, Derbyshire, and Yorkshire, they are generally found only in the upper regions. Mr. Binney in writing of the marine shells in the Lancashire coal field, (*Man. Geol. Trans.*, vol. 1, p. 88,) comes to the conclusion that the fresh water beds are superior to the marine, and that hitherto no bed of the unio, or any other shells of fresh water origin, have in that coal field been found lying between or amongst the beds in which the *Pecten* occur. The presence of these fresh water shells often in great abundance, as at Fitzgerald's colliery near Manchester, sometimes open, and the valves connected by their ligament, preclude the possibility of their being drifted from a great distance: nor is there difficulty in conceiving such a condition of the earth's surface during the carboniferous era, as should lead to both of the results above named—viz.: an alternation of marine and fresh water deposits, and a gradual change from the former to the latter. Here also it may be remarked that an estuary might be of such form as to approximate with almost any degree of closeness to the character of a lake; its extent and depth of water could be such as to give to it all the quietness requisite for the formation of our true coal fields; which appears essential if we sufficiently consider the striking uniformity of thickness in the coal seams themselves; their great continuity and extent even when extremely thin, not exceeding 12 inches in thickness, though sometimes extending many miles; and also if we take into account the regularity as to dimensions in thickness of the strata separating the various beds of coal.

Though some of our coal formations may properly enough be termed basins, there is no type of this kind common to the whole. Coal is sometimes in basins, sometimes flat, and at other times shelving (for an instance of the latter position see Mr. Weaver's account of the Clare Coal Field, *Geol. Trans.*: vol. 5, part 1, second series.) Neither are the organic remains of the different coal fields always alike. Mr. Witham states that the remains of cryptogamia exist in great profusion in the Newcastle, Durham, and Yorkshire coal fields, while in the Edinburgh and Lothian basins imprints of ferns are so rare as to be reckoned curiosities; and in his work on fossil vegetables (p. 9), he states that, "if we take it for granted that the coal seams are formed by deposition of vegetable matter, produced either on the spot where it is now found, or brought from a distance, we can easily offer an explanation for the differences found to exist between the coal fields of England above alluded to, and the Scotch basins, in regard to the occurrence of fossil vascular cryptogamic plants and their impressions. In a (comparatively) flat country like Northumberland, Durham, and Yorkshire, surrounded by mountains of no great elevation, from which a supply of more perfect wood could have been obtained, the vast mass of carbonaceous matter de-

posited must have resulted from vegetables growing on the spot, and this may have had its origin in a great measure from the vascular cryptogamic plants, which a marshy country, such as it might have been, would have produced in great abundance, and with a luxuriance of which we can now have but little conception, unless we contemplate the profuse vegetation of the tropics.

“The Scotch coal basins, on the contrary, seem to have been formed in large inland lakes or hollows, produced by the expansion of immense bodies of water. Into these lakes or hollows, the produce of vast forests, which may have existed in the valleys of the high regions, may have been carried down by eddies and currents. As these trees had grown at great elevations, most of those carried along by the great rivers and their tributary streams may have consisted of coniferæ, or plants possessing a structure closely allied to that of our present pines.”

If we turn our attention to the other theory propounded on this subject, viz., that of local growth, some of the difficulties that have been ascribed to the drift theory appear easily got rid of. Thus the entombment of a mass of vegetation akin or analagous to our own deposits of peat by the superposition of a layer of fine silt or sand, would secure regularity in the line of separation between the two, and also an impression of the vegetable forms that clothed the surface of the field, and were these ferns it would be the fronds only that would be thus moulded; for the stems sinking deep into the vegetable matter would have become incorporated and lost sight of in the bituminized mass.

But we know on the other hand that large drifts of timber do become clothed with vegetation, and supposing this to have occurred previous to such drifts settling to their final resting place, which might not occur for some years, not in fact until the specific gravity of such drifts became greater than that of the water on which they floated, then the fossil remains found at the roof of our coal seams are as easily explained by this theory as the other, and it also as readily accounts for the roofs of our coal seams being a plane surface especially if we remember that before such drifts were covered by the next layer of earthy matter, chemical changes to a considerable extent might have taken place in their constituent elements. On this subject Captain Basil Hall, speaking of the American rivers, says, “In the spring, or rather winter, when the freshes or floods come down, they bring with them millions of trunks of trees technically called ‘logs.’ In February or March, the quantity of these logs is sometimes so great, that not only the river itself, but the sea for several miles off, was so completely covered over with them, that it required some skill to get through. The whole ground, if the loose and muddy soil can be so called, appeared to be formed of layers of these logs, matted together into a net work, or rather a gigantic raft of rough timbers, many yards and perhaps fathoms in depth, over hundreds of square leagues.” And speaking of the

River Atchafalaya, Captain Hall adds, "The river just mentioned flows out of the Mississippi at a point about 250 miles from the sea. Twenty-seven miles from the efflux the raft begins, and extends over a space of twenty miles: but as the whole distance is not filled up with timber, the aggregate raft is only ten miles long. The width of the Atchafalaya is 220 yards; the raft extends from bank to bank, and is supposed to be about 8 feet thick. It has been accumulating for more than fifty years, and is made annually larger, by the supplies of trees drifted into the river from the Mississippi." Referring to the foregoing facts the author of the "History and Description of Fossil Fuel," (a valuable work lately published on that subject,) p. 72, says "In all likelihood this immense aggregation of drift wood will continue to increase, until in process of time it may become covered with sand, and afterwards with vegetation of the higher order, *as already at certain seasons it is gay with verdure and flowers*, and after a succession of ages, present either a seam of coal, or at least an accumulation of the analogous lignites."

Were coal formed in the manner suggested by the late Mr. Bowman in the article before referred to, that is by a series of subsidences and a gradual silting up of shallow waters, then we ought to discover evidences of this operation in the strata just above and below the seam of coal. If each successive seam originated from the waters shoaling up until a surface was presented on which forms of vegetable life could lay hold and fix root, then there would be a point just above the change of water to marsh, when certain forms of vegetable life would prevail and ought now to be discovered, and the forms of vegetable fossils at the floor of a coal seam should differ from those at its roof, except in those cases where the submergence of the seam had been just as leisurely a process as the previous silting up, a condition that might sometimes apply though not to be inferred as a general rule.

Now on this point we can derive considerable information from a valuable paper read by Mr. Binney to the late Meeting of the British Association at Manchester, with the manuscript of which he has kindly favoured us: and in which he appears to have directed special attention to this point. The result of his inquiries plainly shows that in the Lancashire coal field there is this marked distinction in the fossils at the floors and at the roofs of coal seams, as will appear from his following statement:—"The last subject which I shall draw your attention to is the character of the stratum on which the coal rests, generally known by the name of the floor. This deposit is always carefully noted by practical miners, who believe that where a thin seam is found on a good thick argillaceous floor full of *stigmariæ*, it is certain to become if followed, a workable coal. These coal floors are of three kinds—the fire clay—the warrant, (a clay mixed with a larger amount of silica,)—and the rock floors. The first are the most abundant, the second frequent, and the last I have found only in two instances, namely, the feather edge coal at Wal-

mersly and Fecit having a rough quartzore sandstone, and the gannister lately noticed. The last named is merely a fine grained admixture of silica and alumina varying from 8 inches to 2 feet in thickness, and always graduating into a fine fire clay at its bottom. *All the seams, with the exception of the feather edge coal yield the stigmaria ficoides in their floors.* I find this to prevail from the thin coal lying among the limetones of Ardwick down to the two beds found between the millstone grits of Gauxholme, near Todmorden, a distance of near 1700 yards in perpendicular thickness. *In the Manchester coal field all the fifteen floors I have seen afford it. In the middle and lower division 69 different beds at least yield it.* The plant generally has its leaves attached to it, and in all the instances of true floors (those at the bottom of the seam) it occurs without any other coal plants being mixed with it. This absence of other plants and the constant characters of the floors, seem to indicate that all those numerous deposits were formerly made under similar conditions. Their constituents show that they have been suspended in water, and very quietly deposited from that medium." Mr. Binney then adds that an examination of several specimens of the floors had hitherto not enabled him to detect any of the silicious coverings of infusoriæ which have been found so abundantly in the muddy deposits of our present rivers and estuaries by Professor Ehrenberg.

It would thus appear that the distinction of fossil remains which generally (on the theory of successive subsidences), might be expected to prevail at the roofs and floors of coal seams does so. We do not consider the fact though as necessarily proving the theory. It is true the authors of the Fossil Flora consider that the stigmaria ficoides grew in soft muddy soils, but it may have been a submarine or subaqueous plant, that only grew on certain soft deposits, or where coal originated from drift, its heavier specific gravity, consequent on its mode of growth might cause it to sink to the bottom before the other vegetables, settling down with the earth and mud that accompanied it in its change of place. The fact of its being found with its leaves upon it, shows that it could not have been drifted far, but it is unnecessary to suppose it to have come from a great distance. It was evidently a plant of frequent occurrence during the carboniferous era, and is found embedded in a variety of material, from the softest clay to the hardest sandstone, frequently isolated from other organic remains, and we think there is great probability, were it not of submarine origin, of its being found in such profusion around the shores of the lakes or bays of that early epoch, as to be subject to be submerged by every fresh, and to be separated in deposit by its greater specific gravity. Our meaning will be better understood by considering what would occur on a fresh entering one of the lagoons on the shore of the Spanish main, around whose margins the red mangrove almost exclusively prevails, the specific gravity of which is so great, that it would be precipitated

to the bottom along with the earthy matter brought down by the flood: to be covered up, in the lapse of time, by the mass of vegetable matter floated into the same lake by the same inundation, but which remained (like the drift instanced in the American rivers) upon the surface, until it had acquired a sufficient specific gravity to sink, and until, as certainly might be the case, its surface had become clothed with vegetation. Or were the *stigmariæ* growing at the bottom of the lagoon (like the corals and sponges of the present day), then they would be submerged and form the floor of the deposit that overwhelmed them.

As we consider that a complete understanding of this question is only to be obtained by a thorough investigation of our coal fields, we will state from Mr. Binney's paper before referred to some of his observations as to the roofs of coal seams. "These" he says, "are of four kinds, 1st, a fine mixture of alumina and silica, with oxide of iron and a slight trace of the carbonates of iron and lime, generally known by the name of blue bind: 2nd, sandstone: 3rd, black shale: 4th, bituminous schists provincially known by the name of black basses. All these deposits vary at different places, even over the same coal, but the deposits themselves generally contain the same kind of fossils. The blue binds are the most frequent, and contain remains of ferns and other coal plants, *stigmariæ*, *lepidodendra*, *ulodendra*, and *sigillariæ*, and beds of the unio and other shells; the three last named plants (especially the *sigillariæ*) being often found standing erect at right angles to the planes of stratification. The upper part of the field does not afford many erect specimens, the middle portion being their chief place of occurrence." * * * "Sometimes they are found with their roots running into and resting on the seam and more frequently the bole of the tree resting on the coal itself without any traces of roots appearing. The *sigillariæ* are by far the most common. I have only observed the *lepidodendron* at Dixon Fold and the *ulodendron* at Standish near Wigan. The *sigillariæ* in the Duchess of Lancaster mine at Pendleton, and the small coal at Dixon Fold, which afford them as abundantly as they could possibly have grown." * * *

"As to the variability of the nature of the roof and the organic remains which it contains, I may instance the feather-edge coal at Birtle Dean near Heywood, the lowest mine hitherto found worth working in the field. At this place its roof is about a yard in thickness, full of shells of the genera *pecten*, *goniatites*, and *posidonia*, mixed with various ferns, *lepidodendra*, *sigillariæ*, *calamites* and other coal plants, while at Walmersley within two miles of Birtle Dean, the same coal is found embedded in a very coarse grit stone rock, composed of quartz and water-worn granites."

"The roofs of coal all show the effect of different currents of water,—1st. The blue binds exhibit every appearance of a moderate current, which although sufficient to overthrow the delicate ferns and other small plants found prostrate in them, was not able to over-

throw the sigillariæ, lepidodendra, ulodendra, and other great trees ; *for it must be remembered that all the upright specimens of stems of trees found in our coal measures are those of large ones.* Sandstone roofs represent exactly such an appearance as a more continuous and violent current over a tract of luxurious vegetation would now produce, viz., prostrate trees lying in all directions, mingled with sand. All the tender and fragile plants have disappeared, either owing to the violent current that laid them low, or the subsequent percolation of water through the former strata."

"The black shale roofs indicate a very gentle and quiet flow of water and show every appearance of having been a long time in their formation, as the nearly total disappearance of plants and the abundance of the unio in the upper coal measures, and of pectens, goniatites, &c., in the lower prove."

"The black bass roofs however in the upper coal field afford evidence of the longest period of time in their formation, as many of them are nearly one entire mass of the casts of cyprides, microcouchi, bivalve shells, and fish bones and teeth. In the case of the bass, over the three-quarters mine at Bradford, the casts of cyprides, microcouchi, and fragments of fishes appear to constitute the whole mass."

Mr. Binney it will have been observed mentions the stigmaria ficoides in his inquiries as occurring chiefly at the floors of coal seams. The authors of the Fossil Flora in the 2nd vol.: p. xiii, mention it as being very abundant in the shale forming the covering of the coal in the Bensham seam, Jarrow colliery, and in a state of perfection which considering the character of the plants, precluded the possibility of their being drifted to that spot. They remark (vol. 2. p. xxii), "From all these circumstances we are compelled to conclude that these stigmaria, were not floated from a distance, but that on the contrary, they grew on the spots where we now find their remains, in the soft mud, most likely, of still and shallow water."

But as we before intimated the presence of stigmaria whether at the floor or at the roof of the coal seams, or whether floated thither or growing on the spot, though consistent with the theory of local growth and of the formation of coal by gradual subsidence, does not prove that theory ; for such facts appear to us to be also compatible with the supposition that the chief mass of vegetable matter forming the coal seams was drifted to its present position.

Having glanced at some of the opinions and instanced a few of the facts relating to coal fields, we will conclude with a few general observations.

In 1760 Dr. Milles in his letter to the Earl of Macclesfield, president to the Royal Society, describing the bovey coal of Devonshire, a formation so recent as to be little more changed from its vegetable origin than the lignites, came to the conclusion that it was of mineral and not of vegetable origin, because he says, "In the first place, there does not seem to be any imaginable cause in nature, which

could bring together such a mass of fossil wood, as is found in this, and other strata of the like kind in different parts of Europe, &c." (Fossil Fuel, p. 69.) We believe there are men still living who might yet reason in like manner with respect to our true coal seams, though they can scarcely be among those who have paid much attention to the subject.

Among geologists generally, we believe, it is pretty well established that, from the anthracites found among the oldest slates, up, through the true coal formation, to the topmost lignites, all are of vegetable origin.

With regard to the particular mode in which the vegetable matter came into the place where it has since become coal, we are of opinion that it may be accounted for on both theories we have been considering: viz., that of drift and that of local growth.

Such seams as that on which the fossil tress at Dixon Fold now stand, illustrate in our opinion the theory of local growth, and are to be ascribed to it; and surely there is no difficulty in conceiving this to have been the case; for if vegetable matter first drifted, then buried, may become coal, the same vegetable matter if buried without being drifted might become coal also. Such mines as that at Fitzgerald's, near Manchester, with its fossil forest of *sigillariæ* still standing erect upon it, it is reasonable to infer are to be accounted for in like manner. On the other hand for aught that appears, some of the lowest mines of the Lancashire coal field, as the gannister, or feather-edge, may have been accumulated from drift; for we think on careful consideration that some of the principal arguments against coal seams having been drifted are not tenable.

The obvious conclusion would appear to be that inasmuch as accumulations of vegetable matter are collecting at the present day, both by drift and by local growth, under circumstances too which, were they submerged, would admit of their being converted into coal, it is to be inferred that the immense masses of vegetation requisite to form our coal were likewise in both ways collected: at all events until better reasons to the contrary have been advanced. For we, at present, see no necessity for favouring any particular or special process of collecting the vegetation requisite to form a coal seam. We think that each coal field or basin will require to have a careful examination in order to explain its own original condition, and that much will have to be determined by its peculiar position, nature of intervening strata, organic remains, and also of the features of the surrounding country during the carboniferous epoch, so far as they are now to be ascertained.

In looking to some of the Scotch basins for instance, their numerous concentric beds and series of stratification, coupled with their general form, are more easily to be explained on the drift theory, than on that of successive subsidences: but still there is no certain progress to be made as to the manner in which any particular coal field originated otherwise than by a much more careful and

minute survey and investigation of it than has hitherto been made. The gradual change in the carboniferous era from marine to fresh water deposits, the alternation (probably about the point of change of these two conditions,) and the admixture of shells and vegetable forms, all show that it is much more likely that different processes prevailed than that one only did so.

In supposing the formation of a coal field by the gradual silting up of an inland bay or lake, we might conceive the lowermost beds to be formed from drift, while the topmost series might be attributed to a process more akin to local growth, as drifting to any great distance into shallow waters does not readily occur as probable, while extensive marshes covered with vegetation would appear more appropriate in such circumstances. Still, in tropical forests we have seen thin sheets of water or lagoons (which formerly may have been of greater depth) rendered quite black with highly concentrated solution of vegetable matter, originating not only from the decay of the vegetation overshadowing them, but greatly increased by drainage of similar matter from all the surrounding forests. Here then bituminous shale or a seam of coal would be formed by both processes going on at the same time—viz: “drift” and “local growth.” And may not something analagous to this have been the very process by which many of our coal basins were formed. The beds formed at the bottom of the lake or lagoon would be chiefly by drift—principally from the vast tracts of country surrounding it, but partially from the vegetable matter growing on its shores—the former supplying the mass, the latter the covering and admixture of minor vegetables, and the more perfect forms of vegetable life.

We see fewer difficulties in this hypothesis than in that of local growth and successive subsidences, as a general rule. We think indeed that to account for the formation of the whole or any of our coal fields by this series of “sinkings” requires only a little more careful consideration to induce us to pronounce it impracticable, though we might admit its partial application.

Let us take for instance (from his paper before referred to) Mr. Binney’s section of the coal through Ashton, Stalybridge, and Mottram, to the limestone shales of Hollin Brook, “it is about 2000 yards in thickness, and contains about 75 beds of coal, amounting altogether to 150 feet.” Now, if these 75 beds of coal were all to be attributed to local growth, and successive subsidences, it would require 75 successive sinkings of the earth’s crust to be performed uniformly over the whole area of the coal field, and through a depth, taking the aggregate of the subsidences, amounting to upwards of a mile, and without dislocation or rent, the dislocations of the coal strata having occurred after the deposit of the whole series.

We must confess that under such a process we are at a loss to perceive how the various beds of coal should be deposited in basin-formed concentric layers at all. If we suppose the “subsidences” originated in a general “settling of the earth’s crust,” then, it is

true, we should expect parallelism in the layers ; but they should be concave not convex to the earth's centre. Or did the subsidences originate in local sinkings of the earth's crust which were greatest at the centre of the field, and did the coal arise from growth at the surface, then we ought to find the intervening strata thickest in that centre, thinning out towards the edges of the field, and the seams of coal like the plates in a carriage spring would have to be of smallest area at the bottom, larger next above, and so gradually increasing in area to the topmost of the series ; whereas most probably the converse of this last requirement prevails, and we know that there is parallelism in the stratification.

Perhaps then it may be to silting up, such as I have instanced in the American lagoons, rather than to subsidences, that we must attribute many of our coal fields. They who have lived amid the vegetation of the tropics will have little difficulty in believing such a process competent to the production of some of our coal fields, notwithstanding their great extent and the enormous amount of vegetable matter requisite to produce their many seams of coal. But there are many proofs that, compared to the earth's primordial state and the vegetation that then clothed it, even the forests of the tropics are stunted and meagre ; for what meaning have the transition series of our rocks if they do not tell us, that, this earth has been long and gradually preparing for the habitation of man, and that there is nothing either unphilosophical or contrary to true inductive reasoning to suppose, if indeed the fossil flora of the earliest epochs do not prove it, that the forests which in those days engirdled this perhaps almost glowing ball, very greatly surpassed every vegetable remain of the present day.

Institution of Civil Engineers, April 11, 1843.—The Artesian Well of Grenelle.

A LETTER was read from the late Sir John Robison, giving a short account of the artesian well at the Abbadoire de Grenelle, Paris. The Abbadoire being at too high a level to obtain an adequate supply of water by the ordinary means, it was proposed, about eight years since, to sink an artesian well within the premises, which proposal having been agreed to, the execution of it was entrusted to Monsieur Mulot. The work having been perseveringly carried forward through many difficulties, the boring was terminated by the auger penetrating the water-bearing strata on the 26th Feb., 1841, when a sudden and violent rush of water occurred, overflowing at the surface of the ground. As the boring progressed, tubes of rolled iron, and subsequently of copper, were inserted to support the sides, the first being $12\frac{1}{4}$ inches diameter, and the lowest about $6\frac{1}{2}$ inches diameter, reaching to a depth of $1794\frac{1}{2}$ English feet. The quantity of water thrown up while the bore remained in this state was about 880,000 imperial gallons per day, at a temperature

of $82\frac{1}{2}^{\circ}$ Fahrenheit; the expense incurred up to this time has been upwards of £12,000 sterling.

Sir John examined the theoretical reasons which had been given for the contortion of the tubes, which had been attributed to violent pulsations in the flowing water acting upon the outside of them. crushing them outwards; he objected to this reasoning as not being in accordance with the laws of hydrostatic pressure, and attributed it rather to mechanical causes arising from the force used in forcing the tubes down the hole, and even more to the violence they were subjected to in being withdrawn from it.

Mr. Cubitt had recently visited the well, and found the water flowing with considerable force through an orifice in the vertical pipe about 8 feet beneath the level of the ground; the nozzle of the orifice, which was 10 inches diameter, was about half filled, and the stream was reported to be supplying about 2500 litres per minute, at a temperature of 82° Fahrenheit. The water was not clear; it deposited a considerable quantity of fine sand, and occasionally stones of about two inches cube were brought up. He was informed that the water had at one period ascended to between 70 and 90 feet higher than the ground.

Mr. Taylor observed, that the temperature of the water nearly coincided with that of the United Mines in Cornwall, which were 295 fathoms, or 1770 feet, deep. The highest temperature recorded there was, he believed, 96° Fahrenheit. It was well ascertained now, by the experiments of Mr. Fox, that the heat was not increased either by the decomposition of the pyrites, or the number of men and horses employed in the mines.

Mr. Enys said, that the experiments by Mr. Fox, as published in the report of the seventh meeting of the British Association, gave a temperature of 92° in the lode at a depth of 290 fathoms, where it was first reached in the cross-cut; but on proceeding along the same cross-cut, at 10 fathoms from the lode, the temperature decreased to $86^{\circ} 3'$, and at 24 fathoms distant it was $85^{\circ} 3'$: this would give a close approximation to the temperature quoted by Mr. Cubitt.

Mr. F. Braithwaite inquired at what depth the temperature began to increase; landsprings were generally at about 52° , and he found the water in wells 600 feet deep usually at 53° or 54° . He had understood that the temperature increased 1° for every 65 feet after a certain depth.

Mr. Enys said, that Mr. Fox's experiments gave a ratio of increase of 1° of temperature in 48 feet, calculated from the surface. He thought that the close approximation of the temperature of the land-springs, and that in the wells mentioned by Mr. Braithwaite, might be accounted for by the rapidity with which the water filtered through the strata of the London basin.

Mr. Taylor agreed that the heat of the water was influenced by the nature of the strata; the Cornish miners, when they were taken to the North Welsh mines, were much inconvenienced by the cold-

ness of the water in the latter, although the depth of the mines in both districts were nearly identical.

Mr. F. Braithwaite believed that his view of the temperature of wells would be corroborated by the coldness of the water in the new well at Southampton, which had now arrived at a very considerable depth, and he understood that the temperature of the water was about 54° .

Mr. Simpson said, that a well at Southampton has been sunk and bored to the depth of 1063 feet; the supply of water was not considerable, and he was not aware that any observations had been made as to the temperature. A well at Chichester has now arrived at the depth of 1013 feet, and is still being carried lower.

Mr. Sopwith contended for the accuracy of the investigations of Count Brenner, on the temperature of two German mines, and of Messrs. Fox, Buddle, and others in England; the differences between the results obtained were so trifling as to induce confidence in the conclusion they had arrived at, which was, that after allowing for the radiation of heat at a certain distance from the surface, the temperature increased 1° for every 50 feet in depth. This law might not hold good in certain local basins, where from the nature of the strata the percolation of surface water was rapid, but in the extensive mining districts it certainly was correct.

Mr. Cubbit suggested that the close approximation of the temperature of the water in the well at Paris, and that observed by Mr. Fox at the same depth in Cornwall, might arise in some measure from the large volume of water in the former, and the rapidity with which it arrived at the surface; whereas in the deep wells which had been mentioned, the water had probably been allowed to cool before the temperature had been ascertained.

Mr. Clarke corroborated the opinion entertained by Mr. Cubitt: in a well which he had sunk to the depth of 540 feet at St. Alban's, he obtained, by an apparatus constructed for the purpose, some water from the bottom of the well, and found it 4° hotter than that which was pumped up from the same well. At the bottom of a well at Messrs. Barclay's brewery, 367 feet deep, the water was 3° hotter than at the water-level in the same well. Local causes frequently affected the temperature of water in wells: he had seen instances of the water being warmer at 60 or 70 feet deep than at 300 feet; but these cases would not influence the general law.

Mr. F. Braithwaite agreed in the influence of local circumstances, in a well at Cheshunt, at a depth of 40 feet, a sulphureous spring issued, the vapour of which almost killed the workmen; and when at last it was built out, the bricks continued so hot that the hand could scarcely be borne against them. Below that point very cold water was met with.—*Mechanics' Mag.*

Steam Boiler Explosions.

AN elaborate article on this subject has recently appeared in a new Flemish journal, published by M. Jobard, director of the In-

dustrial Museum at Brussels, from whose pen also is the article in question; which has had its origin in the occurrence of a frightful accident from explosion of the boiler of a high-pressure engine at a distillery at Vieux Waleffe, in Belgium, which levelled a large portion of the buildings, and cut through others, leaving a track of desolation such as would have resulted from the firing of a mine beneath the premises.

Perhaps there are few branches of steam-engineering, upon which more has been said than upon this—much to the purpose, and by men well qualified to judge—but much also by persons, more or less devoid of that extended acquaintance with physics and mechanics, and that exact and patient habit of induction, and deduction, which are requisite to unravel the truth out of the tangled and distracting details of each catastrophe as it occurs; and, who, without opportunity of personal scrutiny, should possess these qualifications in a still higher degree, to enable them to approach more exact and generalized knowledge of steam boiler explosions than we possess, by discussion of the observations and frequently vague and contradictory accounts of others. Accordingly, Arago, Perkins, Marestier, Seguiet, Parkes, Combes, Schafhautel, and a host of minor names, have come before the public on this subject; and yet it must be confessed that, at the present hour, the opinions of engineers greatly differ, and are wholly undecided as to several of the supposed causes of explosions, as well as upon what is the prevailing cause, and what may be the best remedies.

That, speaking very generally, want of skill in original construction upon known and acknowledged principles, and want of sound workmanship and materials, are the principal causes of boiler explosions, there can be no doubt. When we just consider the fact, that with our own British steam-power on land and sea, perhaps four or five-fold that of all the world besides, we have not as many explosions in the year by a great disproportion as they have in America, or even in France and Belgium, where the legislatures have stepped in and prescribed various modes, according to which boilers shall be constructed and worked, and overlooked them by domiciliary visits of appointed special officers—it can scarcely be doubted that the great difference in our favour must arise from our better materials and better constructed engines and boilers, and from the greater familiarity of even our lowest stokers with the machines they work, than is found abroad; and also, from our general preference of the condensing to the high-pressure engine, at least, for navigation.

Yet, where are more gigantic high-pressure boilers to be found than those of the Cornish Expansive Pumping Engines, working generally at from three to four atmospheres above the barometer pressure? But here the principle of slow firing, which precludes sudden and dangerous surcharges of steam, is their safety. Let a Cornish boiler be worked with fir logs, as in America, and the results would soon be sadly different.

The whole of the causes of explosion which have been from time

to time stated, may be classed themselves under two general heads: 1st, they have either arisen from the excessive pressure of surcharged steam; or, 2nd, they have taken place through some supposed decomposition going on within the boiler, generating an explosive mixture of gases, which on ignition has produced the accident. Again, the former class has been divided into those arising from the gradual accumulation of steam pressure, where no means of escape has been afforded to the pent-up vapour, until the boiler has riven, and into those supposed to arise from a sudden and almost explosive generation of steam, instantly bringing up the pressure and volume of steam generated beyond the control of the safety valve, and hence again explosion.

In accounting for all these—in showing how safety-valves may and do stick—how *electricity* may gag them down—how soft and hard boiler sediments may cause sudden exposure of red-hot surfaces of plates to the water—how water boiling low, and waving up the sides—how sudden sinking in the internally convex bottoms of waggon-boilers, at a red heat, may do the same—how gauge-cocks become useless, and feed-pumps fail; and in devising and proposing remedies, or assumed ones, for all these real, and (many of them most) improbable evils, philosophers, engineers, legislators, amateurs, chemists, and smoke-doctors, (chemical and mechanical,) have jostled one another, and long amused, but little benefited the public.

While, however, so much has been said and written on this first class of our explosions, very little, in comparison, has been brought forward relating to the second; or those supposed to be attributable to the ignition of inflammable gases: and what little has been said, has always hitherto been dismissed without much examination by men of any pretensions to exact science, upon one obvious and simple ground. It has been said—granting that water is decomposed by iron at temperatures much below visible redness, and that a boiler becomes filled with the hydrogen so evolved, it is equally true the oxygen of the water has been, by the premises, absorbed by the water; and hence, although an inflammable gas is present, an *explosive* one is not. Further, it has been said, evidence is yet wanting that iron will decompose water at any temperature below a bright red or rather yellow heat. These views, in all their collateral circumstances, have been discussed by Baron Seguiér, in the *Annuaire* of 1830. He has shown that the small quantity of air evolved by the water pumped into the boiler, and which goes out again into the cylinder with the steam, will be insufficient to answer the question, and up to a very recent period the question has remained unanswered, if it even be so now.

The Memoir, which forms the subject of this article, is the production of a man obviously fully acquainted with the history of his subject, and not destitute of proper *vigour* of investigation; and if he has not answered fully the question above, he certainly has thrown additional light upon it of a very important kind. The whole would be worthy of translation and perusal, as a good *precis*

of all, or all that is important nearly on the subject. The chief result and novelty brought forward, however, is thus stated in the author's words:—

“Principal Cause of the Explosion of Boilers.”

“When the feed-pump ceases to supply water (the engine being at work) it may *still pump air*, and when thus at any time the boiler ceases to be supplied with water, and its sides become heated by the flame, they decompose the vapour of water present, the oxygen of which unites to the iron, while the hydrogen, *mixing with the air thus introduced*, produces an explosive mixture more or less complete, which takes fire at the red-hot sides of the boiler, or by means of an electric spark occasioned by the raising of the safety valve, which acts the part of an electrophorus, in this case.”

Now, here a sufficient cause is undoubtedly found for the introduction of air. We all know that, from various causes, feed-pumps sometimes cease to pump water, yet still continue to act as condensing syringes for air. This is always partially the case, when the water-feed of a high-pressure boiler is regulated by a cock in the suction-pipe of the feed-pump, as is very common in the North of England. Whenever the plunger gland is ill stuffed, and the cock nearly closed, some air will be drawn into the pump at each stroke; and part of this pumped into the boiler, if it do not escape back through the gland.

But a more general case is, that the supply of water to the feed-pump fails, and the pump still in good order, continues to pump air, or a leakage may exist in the suction-pipe, if the cock above spoken of is leaky.

It is not to be concealed that the cock in the feed-pump suction, being nearly closed, *infers* that the boiler is fully supplied; but it often occurs, that the feed is regulated by the stoker's hand and not by the float. This much, however, is certain, that the feed-pump being out of order, and the water being low in the boiler, are necessarily connected, and hence, that the pumps delivering air in place of water, at the time that the water becomes low, are circumstances probably of co-existent contingency.

If we grant this, it remains to be considered how far three other conditions will admit of our author's solution. 1. Will an explosive mixture of air and hydrogen remain explosive when mixed with a large proportion of undecomposed steam? 2. How much air is the minimum that will cause explosion? 3. Will any degree of heat to which the boiler sides are ever in reality exposed be sufficient to cause ignition?

As to the first, the writer was long amongst those who utterly disbelieved the fact that steam was ever decomposed within the sides of a boiler. He doubted it, arguing from the conditions requisite to decompose water, when passed drop by drop through a red-hot gun-barrel, as well known to chemists. Here it is certain a very bright red heat, or rather a yellow heat, is necessary; and even when water

is decomposed by red-hot charcoal, or coke, a white heat is required: this may be familiarly seen at any foundry; on raking out the cupola, after the day's work, water is thrown on the cokes to quench them; on its contact with the white hot, or rather *blue hot* cokes, a vast volume of flame of liberated hydrogen is instantly produced; but the moment they have fallen below the white heat, all decomposition of the water ceases, and steam alone is generated, or, if hydrogen is produced, it ceases to inflame.

Such, however, does not seem to be the case, where a considerable volume of steam, *under pressure*, and at a high temperature, is exposed for a length of time to contact with a surface of iron, heated even to a very moderate degree. When the temperature of the latter does not even reach redness visible in daylight, decomposition appears, beyond doubt, slowly to proceed.

The conditions, it is to be remarked, are different from those previously stated. Here we have water not as a fluid, carrying off a large quantity of heat in a latent form from the surfaces on which it falls at the moment of decomposition, but water already in the state of vapour, in its state of greatest division; it is not *simply* in contact, under the atmospheric pressure, but it is forced into contact with the iron of the boiler, under a pressure of perhaps several atmospheres. The steam *itself* is at a high temperature, as well as the decomposing surface of iron. Whether that peculiar *description* of molecular action, known to French physicians under the name of "cementation," may here have place or not—whether any *small portion* of the steam once decomposed, (whether by the action of the iron alone, or aided by any reaction introduced by the mixed atmospheric air,) acts as a carrying agent in promoting the decomposition of the remainder, in the same way as the particles of carbon are carried by those of the iron, from the outside to the inside of the still solid bar of iron during its cementation into steel—is a question we have as yet no means of answering; but it is no longer a matter of doubt, that water in the state of steam is, under such circumstances, decomposed. Of this we will adduce one or two instances.

In the small high pressure steam-pipe, or hot water pipes, as they are misnamed, of Perkin's system, inflammable gases had been repeatedly observed to issue from the screw plug at the upper part of the apparatus, when it was replenished with water; on its removal, indeed, the need of such occasional replenishment, where there was no leakage of steam, might have raised the question of what became of the water? But when a year or two since one or more warehouses, and the Museum at Manchester, were either set fire to, or in danger of it, by these said pipes, and an inquiry was instituted, and certain experiments conducted by, the writer believes, a Mr. Davies, of Manchester, he found that in all cases the water or steam, or both, in these pipes were decomposed, and an inflammable gas produced. The temperature of the surface of the pipes appears to have rather exceeded the melting point of lead.

Mr. Goldsworthy Gurney has related similar circumstances as to a high-pressure boiler in actual use.

In February, 1837, a boiler belonging to Mr. Samuel Stock and Son, at Heaton Norris, near Manchester, was emptied by blowing off over night; the main lid was removed next day, and a man attempted to enter with a candle, when a violent explosion is stated to have taken place, and the man was killed. The boiler was found dry and cold. This is related by Mr. Parkes, who adds, that the air had entered and diffused with the hydrogen through the blow-off cock, which was left open.

A similar case occurred at the sugar-house of Rhodes and Son, in London; and about two years ago the writer himself witnessed a similar fact in the case of a high-pressure boiler, of the locomotive construction, with iron shell and brass tubes. The water got low, and was first perceived by the solder (plumber's solder) beginning to melt off in large quantities. About twenty feet of copper steam-pipe attached to the boiler, and the piston rods of the engines becoming blue, the engines were stopped, and the stoker very discreetly drew the fire instantly. The vapour which escaped at the guage cocks was nearly colourless, slightly brownish, from the pressure of charred organic matter, and smelt of impure hydrogen; it was not explosive, but was inflammable. The feed pumps had been working right, and the water was at its proper level, a few minutes before: when the boiler had cooled, it was opened, and two rows of tubes and the whole top of the interior fire-box, were bare of water, but no part of the boiler had been red hot except the brass tubes.

These facts are sufficient to prove that an inflammable gas is produced in the conditions to which boilers are sometimes reduced. The question is, will it be inflammable or explosive when mixed with a certain volume of steam? Upon this head we have no very decisive experiments, or at least such as can be reposed upon.

Dr. Schafhautel (Trans. Ins. C. E., vol. 3, part 5) states, that he has found by experiment that the explosive mixture of one volume of hydrogen and two volumes of air was no longer explosive when mixed with *seven-tenths* of its own volume of steam; that is, a little more than *three-tenths* of the mixed gases in a given volume of steam will be explosive. If these statements are to be relied upon, there is ample range for explosion in practice.

The next question is, what is the quantity of air that must be supplied before an explosive mixture will be generated in the boiler? Here we are most in the dark.

Two volumes of hydrogen and one of oxygen, or between two and three volumes of atmospheric air, are requisite for perfect combustion with detonation; but explosion will take place, even at common temperatures, at much below this quantity of oxygen or air; and when both gases are at a temperature of probably from 300° to 600° Fahr., explosion would probably take place with even less air than at common temperatures.

Pure hydrogen, which we may assume practically to be that now

in question, will probably not fully follow the law of fire damp in this respect. As to the latter, Davy found that the limits of explosion were between five and fourteen parts of air to one of gas, and that the most explosive mixture was seven or eight parts of air to one of gas. He also found that the explosive mixture of fire damp and common air, at maximum was only about *one-tenth* as powerful as that of pure hydrogen and air, and the latter is what is produced in boilers. Further experiments alone, therefore, attending to all the conditions, can decide what are the limits to the quantity of air necessary to be introduced to produce an explosive mixture; upon this quantity the *chances* of its production by our author's "cause assigned," will depend. It should also be mentioned, that Davy found that one part in seven of azote or carbonic acid prevented the detonation of these mixed gases. The last question then is, will the temperature to which the plates of a boiler are ever likely to be exposed in practice, be sufficient to cause ignition?

In this, as in the former case, we want direct and careful experiments, giving heed to *all* the *conditions* of the question. It may be observed, however, that hydrogen and oxygen will begin to unite silently at a temperature so low as 660° Fahr., if sharp fragments of glass, or any other foreign body, be present; and that the presence of clean metals has a powerful effect in lowering the temperature at which explosion may take place. Hydrogen, at common temperatures, is ignited by a glass rod at a low red heat, and at a lower temperature by a metallic one; and it seems quite likely that, if under pressure, and the gas itself heated highly, it would kindle at a much lower temperature still: the same will apply to its mixture with air *pro tanto*. Boiler-plates are every day heated to a pretty bright cherry-red, even on board steam-boats, as the "coming down" of the fire-places sufficiently shows. Hence, although exact information, to be had by exact experiments, is most desirable, there can be little doubt that circumstances may and do aim to sustain the hypothesis of M. Jobard; and that we are justified in concluding that, amongst the *veræ causæ* of boiler explosions, henceforth may certainly be reckoned that which he has apparently been the first prominently to point out, viz., the contemporaneous decomposition of steam or water in the boiler, and introduction of air to its interior. That this however can be but a rare and occasional cause of explosion there can be no doubt; and therefore his conclusion that this is the *principal* cause cannot be admitted: indeed, in Great Britain it is probably a case of the extremest rarity, from the causes before assigned for the general infrequency of our explosions.

The grand and principal cause, which must press upon the mind of every practical engineer, lies in the rupture of the boiler, by the simple accumulation of mere steam pressure with greater or less rapidity; and this may arise from a great number of contingencies. The most usual are—forcing boilers of inadequate size, by rapid and intense firing, that is, using fires wholly disproportionate to the size of the boiler, or whose fire surface, as in the locomotive boiler,

is very great, (in either case, any sudden and unlooked-for stoppage of the efflux of steam gives a fair chance of explosion)—insufficient size of safety-valves, and ill-made or disordered boiler fittings; negligence on the part of those in care of the boiler. For these, and a thousand and one, possible other causes of explosion, as many recipes of prevention have been proposed; but to those who employ engines, and are not themselves engineers, one very simple one is the best. Get your engine from a maker of first-rate character; act implicitly by his designs, and employ sober and trustworthy men to take care of the well-arranged machinery so obtained: the chances of explosion here are very small indeed. For the special cause of explosion adduced by M. Jobard, he proposes a special remedy, namely, that the feed-pump should always work (of course, he means in land engines,) out of an open cistern, exposed to the view of the stoker, so that he could see at once if the water ceased to be drawn away in the pump, by the supply to the cistern causing it to overflow. A better and neater arrangement for the same end would be, to use a close cistern, with a gauge float, so as to prevent dirt (the great cause of feed-pump derangement) from falling into it.

But neither this nor any other possible contrivance can replace a due degree of attention, and a mistake upon this head seems to vitiate, from the very beginning, the hundreds of projects—some ingenious, and most absurd—submitted continually with so much parade, “*à la commission des rondelles fusibles*” of the Institute of France.

It may be remarked, in passing, that the “*grand resultat*” of this learned commission, the said “*rondelle fusible*” itself, seems now to be found by them to be, if not a perfectly worthless, at least a most trustless and deceitful contrivance, and even capable of producing explosion to those reposing in fancied security upon its use; for it is found that, by long exposure to high-pressure steam, the plates of fusible alloy gradually *exude* their more fusible constituent metal, and thus cease to be steam-tight; but before this has happened, the fusing temperature of the “*rondelle*” has, of course, been raised some degrees; and any one, who examines a table of temperatures and corresponding elasticities, will see how small an increment of temperature, in high-pressure steam, co-ordinates with an enormous increase of pressure.

Before taking leave of this subject, the writer would allude to two circumstances bearing less directly upon it. Mr. Parkes, in an article on what he calls the “Percussive action of Steam,” published in the third volume of the Transactions of the Institution of Civil Engineers, part 5, at page 424, after stating his opinion, that most boiler explosions are characterised by suddenness, and few exhibit signs of the operation of a gradually increasing and continued force, adds, “We have examples of boilers weighing many tons, ... being projected to a vast height or distance.” I am unable to comprehend why the simple elastic force of steam can be made to separate a boiler into two parts, projecting the one

into the atmosphere and leaving the other quiescent. . . . I have greater difficulty in understanding how the character of a projectile can be given to such a body, without its having received originally a sudden impulse, using that term as implying momentary action in contradistinction to continuous pressure." This certainly seems a strange conclusion, a riding of the "percussive principle" to death. Is a sky-rocket in its ascent indebted to any sudden impulse? Or Barker's Mill, or, the "Turbine," to any sudden impulse? Or the upsetting of a tall upright jar full of water, by the reaction of a jet of water issuing out of its side? On the contrary, are these not all cases of simple unbalanced continuous pressure? But a circumstance which occurred sometime since upon a railway, with which the writer is acquainted, and the results of which were witnessed by him, set the matter in a very clear light. One of the oldest locomotives the company possessed, with a spherical copper fire-box, had the interior of the latter worn so thin by constant use, that when reduced to about one-eighth of an inch in some places, it burst by bulging inwards while the engine and tender were standing on the rails, steam up, and at the ordinary pressure of about 60 lbs. per square inch, and blowing freely off at the safety valves.

The fire-box burst inwards, as stated, and the steam and water rushed out (like the gas or flame from the tail of a rocket) against the ground. The locomotive engine was instantly thrown (head over heels, so to say,) upon the tender, tearing asunder the large connecting link. Fortunately, no men were on or near the engine, and so no one was hurt.

Now here was a gentle and gradual accumulation of pressure, which though not more than usual, was at last sufficient to rupture the copper plate; and the *unbalanced pressure*, reacting upon the boiler, "projected it into the atmosphere." And so this matter is accounted for without any occasion for calling on the "percussive principle;" the precise physical meaning of which, be it remarked, it is exceedingly hard to see, and the evidences of which, in the various instances brought forward by Mr. Parkes, the writer, after a deliberate consideration, affirms (with every respect for that undoubtedly able and persevering gentleman,) are incapable, when rightly interpreted, and in accordance with the established laws of dynamics, of proving any thing novel, or beyond the well known and perfectly understood effects, of a constant force producing accelerated velocity in a given mass.

One word more. Amongst the many "recipes to produce an explosion" of a steam-boiler is one, which, although the writer has seen it noticed in an incomplete way, and he believes in this Magazine, yet he has not hitherto seen it anywhere placed in the prominent position he believes it ought to occupy—he alludes to *sudden or violent agitation* of the boiling water within a boiler, producing an instant surcharge of steam. For example:—

Take a common Florence flask, about half full of water. Boil the water over a lamp; when all ebullition has ceased, holding the

flask steady, cork it. Take it now by the neck, in the hand, protected by a cloth, and give it one or two vigorous shakes. The flask will instantly burst, and with a pretty observable explosion.

Take another instance. He who lies sleepless in his berth, on board a steam-boat, as in a gale—head to wind, the engines making perhaps twelve strokes per minute, the ship rolling from side to side, and steam constantly blowing off—will hear, at every roll, a constantly recurring sigh of escaping steam, louder considerably than that which is heard between the extremes of vibration. What is the cause of this? It may be said, the safety-valve, just held in suspension by the escaping steam, presses less upon the issuing column when at the extreme roll, than vertically over it. This is true; but the arc of vibration is too small in most of the sea boats to allow of this accounting for the whole phenomenon. The truth is, the steam is now generated in fits. A gush of steam takes place, as at each roll the huge wave of water inside the boilers is transferred across them, and lashes their inner sides—and hence the greater sound.

Take another. Whoever has observed a locomotive “backing up” to a train of carriages, to “hook on”—the steam just blowing off a little from the safety valves—will have noticed that, at the moment the buffers strike, a sudden gush of steam rushes out of both valves. The “percussive action” of the buffers here knocks the water about in the boiler, and, like the water shaken in the flask above, produces a surcharge of steam.

Take a fourth instance to show that the effect of this will be much more formidable when the boiler is full of air, as at first starting, after fresh filling. Brewers, to “sweeten” their casks, pour a little scalding-hot water into them, say, a gallon or so into a cask holding thirty or more. A man bungs up the aperture, and, seizing the cask, rolls and shakes it about in every direction. In a few seconds, if the bung be not *blown* out, he pulls it out, or rather starts it, when it flies out, accompanied by a very considerable rush of steam and air. Now the reason of this curious fact is obvious enough to any one acquainted with the elements of this branch of physics; so we need not stop to discuss it.

Here, then, we have a real cause frequently in operation, both on land and at sea, for sudden surcharges of steam, when every part of engine and boiler is in perfect order, and often sufficient to endanger or destroy the former.

It is a remarkable fact that the great majority of steamboat explosions which have happened, both in America and in Great Britain, have taken place at the moment of starting, after the boat has been “laying to.” The explosion on the Clyde, a year or two since, and the latest American instance, were both so circumstanced. The few locomotive boilers, also, which have ever exploded while sound and in good condition, have “gone off” at the first moment of getting into motion: as, for instance, that which exploded in the station-house at Liverpool, some years since, in the act of backing

up to a train. On the degree of danger really attributable to this cause, experiments are wanting, as well as on many other relating questions to this important subject, upon which the only experiments, of any real practical value, made as yet, appear to be those of the Franklin Institute, which, it is to be regretted, are so little known in these countries, and do not embrace the whole subject. Experiments, however, to be of any decisive authority, or practical value in the matter, must be made upon a large and practical scale. Experiments made on the laboratory table, and of the class of those which, from the combustion of a grain or two of potassium, dropped into water in a glass tube, are made the groundwork of deductions as to forces when tons of matter are put in motion, are only calculated speciously to mislead.

R. M.

Dublin, Feb. 11, 1843.

[*Mechanics' Mag.*

Photic Fluids—The History of Chemistry.

Sir,—I have great pleasure to say, that I consider your correspondent "Cogito's" second letter in reply to my answer to his first comment on my hints concerning matter, perfectly satisfactory, and such as I am bound fully to appreciate and to pay particular attention to. I confess to you, that his first letter had made me entertain some doubts as to the friendliness of his remarks, while the second proves to me the contrary. I can assure him, that the objections which he now makes are exactly what I was prepared for, and so much prepared for, that the dread of having to combat them had prevented me from stepping publicly forward with my theory until you drew my attention to Dr. Möser's experiments. The perfect coincidence of the facts which he sets forth, with my own conviction brought me into the field, or rather pushed me into it, "Cogito" now states clearly, where he has understood me, and where not; and that by the "Antagonist Essences" he meant the "Phlogistic" theory, which he conceives to imply a double machinery; at the same time acknowledging that the "centripetal and centrifugal tendencies" set forth by me "*were not then thought of*," nor were thereby elucidated. He further admits one great point, namely, the "*existence of photic fluids*" so that the question about them at issue between us, is no longer, *that* they are, but *what* they are. Thus there now exists an approximation between us, in some respects at least, and I am not only enabled, but also bound to argue with him concerning the points on which we yet differ.

In his summary of my propositions he overlooks one main feature, alluded to in my first letter, namely, that the contents of what we call a *vacuum*, or exhausted space, (provided it really be exhausted of elemental matter, may be identical with the ether impregnated with photic fluids. It follows from this stipulation of mine, that we are probably able to examine the nature and properties of imponderable fluids *in nudibus*. I therefore request him to make this addition to the first passage of his summary.

"Cogito" then says, that he does not comment on the first two propositions, such as he states them, because he does not yet sufficiently understand them. It is impossible that he should, for as yet I have neither explained, *how* I got by those principles, nor shown by what specific laws they are connected with each element; nor can I do so, till I give my system in detail. The further discussion of these questions must therefore remain in abeyance for the present.

In regard to my assumption of a six-tuple photic fluid, he thinks that to be *old*, in the first place, and erroneous in the second. In reply, I can assure him that I was not aware of its being old; for I have nowhere met with an enumeration of six photic fluids in any shape, much less in that of their identity with the four electro-magnetic polarities; combined with distinct black and white light and an ether. Therefore I, at any rate, have not derived them from any *old* scientific theory that may exist unknown to me. As to their being erroneous, we are in the same predicament as in regard to the other questions.

I am, as above said, glad to learn that by the "Antagonist Essences," he meant the phlogistic theory; for on the one hand, this is intelligible to me; and on the other, I comprehend also, that he could not understand any part of my propositions, as long as he did not see the difference there is between them and the phlogiston of old. It is the object of the present letter to point out that difference, together with my own position, as a necessary preliminary to all further progress.

May I then be allowed to state, that in my studies I have chiefly applied myself to some of the *newest* English and foreign works on chemistry and astronomy, and that the only account I have seen and wished to see, concerning the theory of phlogiston, such as propounded by its *founder*, is contained in the first volume of a very able work, the Chemistry of Dr. Kutzinger, of Nordhausen, published in 1838, but which I only happened to read *after* I had established my principles in my own mind. The account I speak of forms part of his introductory sketch of the History of Chemistry, which he divides into four periods, namely, *the alchymistic, the phlogistic, the anti-phlogistic, and the electro-chemical*; the last being that in which we still are. After having duly despatched the absurdities and mysticism of the long-lived *alchymistic* period, he says with regard to the second, or *phlogistic*,—

"It was founded by Dr George Ernst Stahl, who was born at Auspach, in 1660, and died in 1776, at Berlin, as physician in ordinary to the king of Prussia, after having previously, for 22 years, been ordinary Professor of Medicine at Halle, and delivered chemical and medical lectures with great applause. His system was founded on the hypothetical assumption of a combustible principle or *element*, which he called 'phlogiston.'" (N.B. He meant a *single* or *simple* ponderable element, convertible into the flame of fire, a thing of which I know nothing.) "According to him everything that burnt, or was com-

bustible, was indebted for this property to the presence of the phlogiston; all metals, sulphur, coal, pitch, oil, wood, &c., consisted of this combustible, (single element,) which in their respective substances was combined with other matters." (N.B. This is a good basis for the theory of undulation, according to which matter has *its own power* to burn and radiate; consequently to get rid of inertia, when in contact with an opponent neighbour!) "Such bodies as were most easily inflammable, according to his theory, contained a greater quantity of the (ponderable) phlogiston than others, which were less easily combustible. Now, when the metals at the melting point, lost their metallic appearance—for instance, lead, tin, iron, copper—and were converted into an earthy or limy mass, which he called 'metallic calx,' he accounted for the phenomenon by assuming that, in the process of melting, the phlogiston escaped from its prior combination with the metals." (I admit no such thing with reference to any element.) "Thus the metals consisted of earth, (calx), and phlogiston. But such earths might again be converted into metals, by bringing them into contact with matter containing much phlogiston, (for instance, coal, oil, tallow, rosin,) exposing them at the same time to a high heat, whereby the latter transferred their phlogiston to the earth, and re-established the metal. Although this theory was subsequently found to be erroneous, yet it was the *first impulse*, and consequently Stahl's merit, to give a scientific foundation to elementary chemistry, and to separate it gradually from the art of *gold-making*, by identifying the latter with chimerical alchemy; for although the metals could be produced from *some* earthy matters, the notion was at last given up, that *coarse* metals could be converted into *precious* ones. In this same period of the famous Stahl, other men also distinguished themselves, whose names science even now records with the greatest esteem. Priestley and Scheele belong to the same period: both of them *independently* of each other, at the same time discovered the oxygen gas, and thereby introduced one of the most important periods of chemistry; for, without this important discovery, that science would still be in its infancy, while since that time it has developed itself with gigantic strides, and now takes up, in natural sciences, the first place next to astronomy. The succeeding periods follow each other so rapidly, that there are still living chemists, who, beginning with the phlogistic period, have fought their way through the whole cycl^{us}.

"In a time when such men as Scheele, Priestley, Bergman, Crell, Boerhaave, and other famous chemists lived, it could not take a long time to discover the fallacy of the phlogistic theory: for if the metals, when brought to a melting state, could lose their (ponderable) phlogiston, they could not but also *lose in weight* by combustion; but the very contrary was the case: the remaining earth, into which the combustionized metal was converted, weighed *more* than the original quantity did. Although the zealous defenders of the phlogiston now endeavoured to save the principle, by ascribing to it a

negative weight, this device only availed them a short time in their dilemma: for it did not last long, till the accurate experiments of Lavoisier dethroned phlogiston, in order to enthrone oxygen in its stead."

Here I beg leave to observe, that this account makes no mention of the *exact* phlogistic theory having originally set forth a principle of *imponderable antagonist*, or *phlogistic* and *anti-phlogistic essences*; and that this, in fact, resembles our present *electro-magnetic theory*, which shows the "positives" to steer and attract one way, and the "negatives" another, thus advocating double electricity and double magnetism, each combined in the centre. Thus science has a double machinery with four poles. Dr. Kutzing continues:—

"The third period is that of *anti-phlogistic* chemistry. It begins with the year, 1783, and was founded by Lavoisier, in Paris, (born 1743.) He proved, by accurate experiments, that metals, being melted by heat, not only *lose nothing* of their weight, but even, on the contrary, *gain by attracting something from the atmosphere*, and that they lose their metallic aspect and consistency by *combining with this something*. This *material something*, which he again succeeded to separate from the metals he called 'oxygène:' while Hermstadt gave it a German name, signifying 'acid matter.' The *fourth or electro-chemical* period begins with the year 1807, when Sir Humphry Davy discovered that the alkalies and earths were decomposable into peculiar metals and oxygen: and he did so by means of the galvanic battery. This last is our present stage."

The original phlogistic theory, accordingly, seems to have been rather different from what "Cogito" presumes it was; and it is, at any rate, far from any similarity with my principles; for the phlogiston was conceived to exist in the shape of a *ponderable element*: combined with others. When it was found out that it could not be maintained in that shape, an attempt was made to translate it into an imponderable lord spiritual, and as such it died. Soon after, the phlogistic idea was in fact resuscitated in the shape of another *double ponderable machinery*, by adapting it to the functions held by *oxygen* and *hydrogen*, as is proved by James Watt's communications to Dr. Priestley: (see Lardner's "Steam Engine," page 307,) carbon, phosphorus, sulphur, &c. became "aides-de-camp." But all this, as far as it went, had nothing to do with photic fluids or galvanism. And still less am I inclined to adopt any *older* notions or principles formed in times, in which nobody yet knew anything about the existence and elementarity of gases, or of *air*, as being *not* an imponderable, but a ponderable compound of volatilized elements, which may be found combined with the hardest metals in a highly dense condition, and which I consider to be merely gaseous when freely floating in, atomically expanded by, and centrally much fraught with, *the ether* that surrounds, presses upon, and pervades the terrestrial atmosphere and body. And pray how long is it yet, *since we knew*, that there is such a thing as space, totally deprived of elemental matter, or an exclusion from

it of the *terrestrial* ponderables? So that there is no more occasion to *assume at a low temperature*, an almost infinite and chimerical rarifiability of air tantamount to imponderability.

As to any general theories of the constitution of the universe, formed prior to the recent discoveries in astronomy and chemistry, I know of none embracing the whole system, and accounting satisfactorily for the real nature of, and the connexion between, ponderable and imponderable matters, and even now the prevailing opinions on many essential questions are still so vague, inconsistent, and contradictory, that an approximation between them seems to be impossible.

I do not, however, despair of its being sooner or later accomplished by joint efforts, and to this great end every reflecting man is bound to contribute his mite as he may, for in this respect even a single hint or new idea, if supported by a single coincidence of known facts, may go a long way towards arriving at general fundamental laws, that may hold good throughout.

I remain most truly, Sir,

Your most obedient servant,

Z.

P.S. I am by no means unacquainted with the scientific tenets of antiquity, so frequently recapitulated by the moderns. The *ancients* had formed *many* hypotheses, and got up many names for mysterious things and notions about things; but they were deficient in radical principles and the knowledge of *experimental facts*, for establishing *truths* beyond a doubt. Thus, for instance, every thing said and written about the law of gravitation or attraction, even by *Newton himself*, prior to 1682, was *old* and amounted to *mere rational conjecture*, until in that year, Newton succeeded in *proving the truth* by *new facts*: by that means, and from that moment, it became a perfect *novelty*, and as *such*, a *great discovery*; which it still took him *four years*, to work it out fully, so as to be able to publish his "*Principia*" in 1686.—*Ibid.* Z.

London, February 4, 1843.

Mr. Ewbank's Book of Hydraulics.—Importance of Mechanical Arts.

"REMARKS have occasionally been introduced on the importance of the mechanical arts, and the real dignity attached to their profession, notwithstanding the degraded state in which operatives have ever been held by those who have lived on their ingenuity, and become enriched by their skill. But this state of things we believe is passing away, and the time is not distant when such men, instead of being deemed, as under the old regime, virtual serfs, will exert an influence in society commensurate with their contributions to its welfare. And where, it may be asked, is there a comfort, or convenience, or luxury of life, which they do not create or assist to furnish, from the bread that sustains the body, to the volume that informs the mind?

"Few classes have a more honourable career before them than intelligent mechanics: certainly none have better opportunities of

associating their names with those of the best of their species. Science and art open the paths to true glory; and greater triumphs remain to be achieved in both than the world has yet witnessed. Human toil has not been dispensed with; but it certainly will be superseded, in a great measure, if not altogether, by forces derived from inanimate nature. A great part of the globe is yet a desert, inhabited by beasts of prey, or by men more savage than they; whereas, the Creator designs the whole to be a garden, and peopled with happy intelligences, as in the first Eden. It is much too common to seek ephemeral distinction on the troubled sea of politics or party; but of the thousands who launch their bark upon it, how few ever reach the haven of their wishes! The greater part are soon engulfed in oblivion, while not a few, exhausted by useless struggles, are bereft of their energies, and quickly sink in despair; but no fame is more certain or more durable than that which arises from useful inventions. Whitney and Whittimore, Evans and Fulton, will be remembered as long as cotton gins, carding machines, steam-engines, and steam-boats, are known on these continents, and when contemporary politicians are wholly forgotten—in fact, most of these are so already. The name of Watt will be known when that of every warrior, and monarch, and statesman of his day has perished; and so it ought to be, for, with few exceptions, he contributed more to the happiness of his species than have such men from the beginning of time. No one is now interested in learning any thing respecting the sanguinary Bull of Burgundy, and his wily antagonist, the eleventh Louis of France, whose contests kept for years the European world in an uproar; and the latter, not content with murdering his species by wholesale, in his old age slew infants, that he might acquire new vigour by bathing in their blood; but as long as time endures the world will revere the names of their contemporaries—Gottenburgh, Koster, Faust, and Schœffer, and their associates in printing and type-founding.

“Science and the arts are renovating the constitution of society. The destiny of nations cannot be much longer held by political gamblers, wealthy dolts, titled buffoons, and royal puppets; these, no longer sustained by factitious aids, must descend to their own level. Theories of governments will not be opposed to nature, and carried out in violation of her laws; but practical science will be the ruling principle; and practical philosophers will be, as God designed they should be, the master spirits of the world. The history and progress of the useful arts will soon become a subject of general study. Historians will hereafter trace in them the rise and fall of nations; for power and pre-eminence will depend upon new discoveries in, and application of, science. Battles will soon be fought by engineers instead of generals, and by mechanism in place of men. But battles, we trust, will hereafter be few; for if ever men were called upon by that which is dear to them and their race—by that which is calculated to raise the purest feelings and exterminate the worst ones—it is to denounce that spirit of military

glory which encourages and induces offensive wars. Take away all the false glare and pomp of wars, and tyranny will expire, for it will have nothing to support it. Put war in its true light, and no well-regulated mind would ever embrace it as a profession."—Pref. page v.

Of what sort the "remarks" are which have called for this prefatory justification, the reader may judge from the following extracts:—

Early History of the Arts.

"The SCHOLAR mourns, and the ANTIQUARY weeps over the wrecks of ancient learning and art—the PHILOSOPHER regrets that sufficient of both has not been preserved to elucidate several interesting discoveries, which history has mentioned; and to prove that those principles of science, upon which the action of some old machines depended, were understood; and the MECHANIC inquires in vain for the processes by which his predecessors, in remote ages, worked the hardest granite without iron, transported it in masses that astound us, and used them in the erection of stupendous buildings, apparently with the facility that modern workmen lay bricks, or raise the lintels of doors. The machines by which they were elevated are as unknown as the individuals who directed their movements. We are almost as ignorant of their modes of working the metals, of their alloys which rivalled steel in hardness, of their furnaces, crucibles and moulds; the details of forming the ennobling statue, or the more useful skillet or cauldron. Did the ancients laminate metal between rollers, and draw wire through plates, as we do? Or, was it extended by hammers, *as some specimens of both seem to show? * On these and a thousand other subjects, much uncertainty prevails. Unfortunately, learned men of old deemed it a part of wisdom to conceal from the vulgar all discoveries in science. With this view, they wrapped them in mystical figures, that the people might not apprehend them. The custom was at one time so general, that philosophers refused to leave anything in writing, explanatory of their researches.

"Whenever we attempt to penetrate that obscurity which conceals from our view the works of the ancients, we are led to regret, that some of their MECHANICS did not undertake, for the sake of posterity and their own fame, to write a history and description of their machines and manufactures.

"We know that philosophers, generally, would not condescend to perform such a task, or stoop to acquire the requisite information, for they deemed it discreditable to apply their energies and learning to the elucidation of such subjects. (Few could boast with Hippias—who was master of the liberal and *mechanical* arts—that the ring

* "And they did beat the gold into thin plates, and cut it into wires." Exod. xxxix. 3. These plates, were probably similar to those made by the ancient goldsmiths of Mexico, which were "three quarters of a yard long, four fingers broad, and as thick as parchment."—Purchas' Pilgrimage, 984. "Silver spread into plates, is brought from Tarshish, and gold from Uphaz."—Jer. x. 9.

on his finger, the tunic, cloak, and shoes which he wore, were the work of his own hands.) Plato inveighed with great indignation against Archytas and Eudoxus, for having debased and corrupted the excellency of geometry, by mechanical solutions, causing her to descend, as he said, from incorporeal and intellectual to sensible things; and obliging her to make use of matter, which requires manual labour, and is the object of servile trades.*

"To the prevalence of such unphilosophical notions amongst the learned men of old, may be attributed the irretrievable loss of information respecting the prominent mechanics of the early ages, those

‘Searching wits,
Who graced their age with new invented arts.’

Virgil, En. vi. 900.

"Their works, their inventions, and their names, are buried beneath the waves of oblivion; whilst the light and worthless memorials of heroes, falsely so called, have floated on the surface, and history has become polluted with tainted descriptions of men, who, without having added one atom to the wealth or to the happiness of society, have been permitted to riot on the fruit of other men's labours; to wade in the blood of their species, and to be heralded as the honourable of the earth! And still, as in former times, humanity shudders, at these monsters being held up, as they impiously are, to the admiration of the world, and even by some Christians too, as examples for our children."

The Discoveries to be yet discovered.

"There is no truth in the observation of some people, that all discoveries of importance are already made; on the contrary, the era of scientific research and the application of science to the arts may be considered as but commenced. The works of creation will for ever furnish materials for the exercise of the most refined intellects, and will reward their labours with a perpetual succession of new discoveries. The progress which has been made in investigating the laws that govern the aqueous, atmospherical, mineral, and vegetable parts of the creation, is but a prelude to what is yet to be done; it is but the clearing of the threshold preparatory to the portals of the temple of science being thrown open to the world at large. There is no profession, however mature, no art, however advanced, that is not capable of further improvement; or that, so far as we can tell, will not always be capable of it. If an art be carried to the utmost perfection it is capable of in one age, discoveries in others will in time be made, by means of which it will be still further advanced; for every improvement in one has an effect, more or less direct, on every other.

"The benefits already derived from steam are but as a drop to the ocean when compared with those that posterity will realize; for if such great things have been accomplished by it in one century, what may not be expected in another, and another? It has been

* Plutarch's Life of Marcellus.

calculated that 200 men with machinery moved by steam, now manufacture as much cotton as would require 20,000,000 of persons without machines; that is, one man, by the application of inorganic motive agents, can now produce the same amount of work that formerly required 100,000 men. The annual product of machinery in Great Britain, a mere spot on the earth, would require the physical energies of one half the inhabitants of the globe, or 400,000,000 of men; and the various applications of steam in different parts of the world, now produce an amount of useful labour, which, if performed by manual strength, would require the incessant exertions of every human being. Hence this great amount of labour is so much gained, since it is the result of inorganized forces, and consequently contributes so much to the sum of human happiness. Now, if such results have been brought about so quickly, and by steam alone, what may not be expected from it, and other æriform fluids, in ages to come, when the progressive improvement of every art and every science shall have brought to light not only other agents of the kind, but more efficient means of employing them? There is no end to the beneficial application of the gases as motive agents, and no limits to the power to be derived from them. As long as rain falls, or rivers flow, while trees (for fuel) grow, or mineral coal is found, man can thus wield a power that renders him almost omnipotent."

The remainder of our extracts shall be from the more technical portions of the work.

Atmospheric Sprinkling Pots.

"The retention of water in *inverted* vessels while air is excluded from them, could not have escaped observation in the rudest ages. Long ere natural phenomena had awakened curiosity in the human mind, or roused the spirit of philosophical inquiry and research, it must have been noticed. When a person immerses a bucket in a reservoir, and raises it in an inverted position, he soon becomes sensible that it is not the weight of the vessel merely which he has to overcome, but also that of the water within it; and not till the mouth emerges into air do the contents rush out and leave the bucket alone in his hands. This is one of those circumstances that has occurred more or less frequently to most persons in every age. It would be absurd to suppose that the groups of oriental females, who, from the remotest times, have assembled twice a day to visit the fountains of rivers for water, did not often perform the experiment, both incidentally and by design. They could not in fact plunge their water pots (which were often without handles) into the gushing fount without occasionally repeating it; nor could Andromache and her maids fill buckets to water the horses of Hector, and daily charge pitchers in the stream for domestic uses, without being sometimes diverted by it. But the phenomenon thus exhibited was not confined to such occasions; on the contrary, it constantly occurred in every dwelling. An ancient domestic, like a modern housemaid, could hardly wash a cup or rinse a goblet by *immersion*, without encountering it. Be-

sides the vessels named, there were others that formed part of the ordinary kitchen furniture of the ancients, the daily use of which would vary and illustrate it. These were long-necked and narrow-mouthed vases and bottles, that retained liquids when inverted like some of our vials. Others were still further contracted in the mouth, as the *ampulla*, which gave out its contents only by drops. To the ordinary use of these vessels, and to incidental experiments made with them, may be traced the origin of our *fountain* lamps and ink-stands, bird fountains, and other similar applications of the same principle.

"The suspension of a liquid in inverted vessels by the atmosphere was therefore well known to the early inhabitants of the world, whether they understood the reason of its suspension or not; and when in subsequent times philosophers began to search into causes and effects the phenomenon was well calculated to excite their attention, and to lead them to inquiries respecting air and a vacuum: it is probable that it did so, for the earliest experiments on these subjects, of which we have any accounts, were similar to those domestic manipulations to which we have alluded, and the principal instrument employed was simply a modification of a goblet inverted in water. This was the atmospheric '*sprinkling pot*,' or '*watering syphon*,' which is so often referred to by the old philosophers, in their disputes on a *plenum* and a *vacuum*. It has long been obsolete, and not having been noticed by modern authors, few general readers are aware that such an instrument was ever in use, much less that it formed part of the philosophical apparatus of the ancient world.

"The interesting associations connected with it and its modifications entitle it to a place here. Indeed were there no other reason for attempting to preserve it a little longer from oblivion, than that indicated at the close of the last paragraph, we should not feel justified in passing it by. It is moreover, for aught that is known to the contrary, the *earliest* instrument employed in hydro-pneumatical researches. Its general form and uses may be gathered from the remarks of Athenagoras respecting it. This philosopher, who flourished in the fifth century B. C., made use of it to illustrate his views of a vacuum. 'This instrument (says he), which is acuminate or pointed towards the top, and made of clay or any other material, (and used as it often has, for the watering of gardens) is in the bottom very large and plain [flat] but full of small holes like a sieve, but at the top has only one large hole.' When it was plunged in water, the liquid entered through the numerous holes in its bottom; after which the single opening at top was closed by the finger to exclude the air; the vessel and its contents were then raised, and the latter discharged at pleasure by removing the finger.

"As this was the ancient *garden pot* of the Greeks, Pliny probably refers to it when he speaks of '*sprinkling*' water, oil, vinegar, &c., on plants and roots. It appears to have been continued in use for such purposes in Europe, through the middle ages; and to a limited extent up to the 17th and 18th centuries. Figures of it are,

however, rarely to be met with, for it seems to have been nearly forgotten when the discovery of Torricelli revived the old discussions on a vacuum; and though Boyle and others then occasionally referred to it, few, we believe, gave its figure. Montfaucon speaks of examining an ancient 'watering stick,' and also a 'sprinkling pot;' but unfortunately he has not described either. Of a great number of old philosophical works, that we have examined for the purpose of obtaining a figure, we met with it only in Fludd's works."

Invention of the Atmospheric Pump.

"It is uncertain when or by whom the common atmospheric pump was invented. It is supposed to have been known to the old Egyptians, and to have been used in the ship in which Danaus and his companions sailed to Greece. As the *antlia* of the Greeks, it could not have originated with Ctesibius, to whom it has sometimes been attributed, since it or some other machine or device is mentioned under that name, by Aristophanes and other writers who flourished ages before him. There are other indications that it was previously known, for either it or something very like it is mentioned under the name of a *syphon*. This term it is known was a generic one, being applied to hollow vessels, as funnels, cullenders, pipes; and generally to instruments that either raised or dispersed water, as syringes, catheters, fire-engines, sprinkling-pots, &c. That the machine to which we refer raised water by 'suction,' is apparent from ancient allusions to it. According to Bockler, 'the Platonic philosophers asserted that the soul should partake of the joys of heaven as through a syphon;' and by it Theophrastus explained the ascent of marrow in bones; and Columella the rise of sap in trees. In these instances, it is obvious that neither the ordinary syphon nor the syringe could be intended, but the atmospheric pump; a machine that Agricola described as a syphon; and one to which the remark of Switzer only can apply—'the syphon was undoubtedly the chief instrument known in the first ages of the world, (besides the draw-well,) for the raising of water.'

"Nor is there anything in the account given by Vitruvius of 'the machine of Ctesibius,' which indicates that the atmospheric pump was not in previous use. His description is obviously that of a *forcing* pump, (and appears to have been so understood by all his translators,) one whose working parts were placed not above but in the water it was employed to elevate; whose piston was solid, and which by means of pipes forced the water *above itself*; that raised the water 'very high;'—attributes which do not belong to the common pump. It is true he has not mentioned the latter, perhaps because it was not then employed as now in civil engineering, and therefore not within the scope of his design in writing his work. The manner in which Pliny speaks of it shows that it was an old device in his time, since it was one with which even country people or farmers (the last to adopt new and foreign inventions) were familiar. In his 19th Book, 'On Gardens,' cap. 4, he observes: when a

stream of water is not at hand, the plants should be watered from tanks or wells, the water of which may be drawn up by plain poles, hooks and buckets, by swapes or cranes [windlass], 'or by *pumps* and such like.' And that these were no other than the old wooden pump of our streets, and such as our farmers use, is obvious from a passage in his 16th Book, cap. 42, where, speaking of the qualities and uses of different kinds of wood, he remarks, 'pines, pitch trees, and allars, are very good to make *pumps* and conduit pipes to convey water; and for these purposes their wood is bored hollow.'

"Although sufficient time may be supposed to have elapsed from the age of Ctesibius to that of Pliny for the introduction of the atmospheric pump to the countrymen of the latter (supposing it to have been invented by the former), we can hardly believe, if it were not of more remote origin, that it could even in that time have found its way into Roman farm yards and gardens; much less that it should have superseded, (as it appears to have done,) every other device on board their ships. New and foreign inventions were neither circulated so easily nor adopted so readily in ancient as in modern days; and even now a long time would elapse before inventions of this kind would find their way through the world, and longer before they became generally adopted. But had the pumps of which Pliny speaks been of recent introduction, he would certainly have said so; and had they been the 'water forcers' of Ctesibius, to which he alludes in his 7th Book, he could scarcely have avoided recording the fact."

Origin of Safety-valves.

"It should not be supposed that safety-valves were wholly unknown before Papin's time; on the contrary, they were frequently used, although this fact has not been noticed by any writer on the steam-engine. The liability of stills and retorts to be rent asunder led old chemists to apply plugs to openings in those vessels, that the vapour might raise or drive them out, and escape ere its tension exceeded the strength of the vessels; such were the plugs in ancient steam deities. In some old works on distilling, conical plugs or valves are shown as fitted into cavities on the tops of boilers, and in some cases they were loaded. In the '*Maison Rustique de Maistres Charles Estienne et Jan Liebault, Docteurs en Medicine,*' Paris, 1574, folio 196, 197, are figures of two *close* boilers, in which the distilling vessels were heated: one formed a water, the other a vapour bath. On the top of each is a conical valve opening upwards. These served both to let out the superfluous steam and to introduce water. Glauber, who contributed several valuable additions to the mechanical department of chemistry, has figured and described, in his *Treatise on Philosophical Furnaces*, the modes by which he prevented glass retorts and stills from being burst by the vapour. A long stopple or conical valve was fitted to the neck of each, being ground air-tight to its seat, and loaded with a 'cap of lead,' so that when the steam became too 'high' it slightly raised the valve, and

a portion escaped; the valve then closed again of itself, 'being pressed down with the leaden cap, and so stopt close.' (English Translation, Lond. 1651, p. 306.) The valve on Newcomen's first engine was of this description. In the same work Glauber describes the most philosophical of all safety-valves, viz., a column of mercury enclosed in a bent tube which communicates with the boiler or still, somewhat like the modern mercurial guage. He also describes that beautiful modification of it known among chemists as the water lute, or quicksilver lute: that is, around the mouth or neck of a vessel a deep cavity is formed and partly filled with water or mercury, as the case may be. A cylindrical vessel, open at top and closed at the bottom, forms the cover; it is inverted, the open end being placed in the cavity and dipping as far into the liquid as the internal pressure may require. In 'The Art of Distillation, or a Treatise of the choicest Spagyricall Experiments,' &c., by John French, Doctor of Physic, Lond, 1751, the author describes the same devices for preventing the explosion of vessels as those mentioned by Glauber. Speaking of the action of such safety-valves he observes, (page 7) 'upon the top of a stopple [valve] there may be fastened some lead, that if the spirit be too strong, it will only heave up the stopple and let it fall down again.' Papin's claim therefore is not to the valve itself, but to its improvement, or rather to the mode of applying it by means of a lever and moveable weight; thereby not only preventing the valve from being blown entirely out of its place, but regulating the pressure at will, and rendering the device of universal application."—*Ibid.*

The Electric Telegraph.

WE copy from the 92nd number of "Silliman's Journal of Science" the following notice of the interesting and very successful experiment made for the purpose of proving the practicability of Professor Morse's Electric Telegraph at very great distances between the stations. In the present instance the conducting wires were *one hundred and fifty-eight miles* long. We learn that the appropriation made at the last session of Congress for laying down the lines between Baltimore and Washington is deemed to be quite sufficient for the purpose; and, further, that the telegraph is expected to be completed and in operation before the next session of Congress is concluded:

"*Professor Morse's Electro Magnetic Telegraph.*—We have been recently favoured with an opportunity of witnessing some remarkable experiments preparatory to the construction of the telegraphic line between the cities of Baltimore and Washington.* Wires the total length of one hundred and fifty-eight miles† having been

* The distance from Washington to Baltimore is 40 miles, and Professor Morse's telegraph, it will be noticed, requires four times that length of wire.

† Two coils of a mile each were in the circuit in connexion with a magnet which made the entire length 160 miles.

prepared, it was thought proper by Professor Morse that this unusual length of wire should be used as a test of the powers of his system, and also as a means of determining such points of scientific inquiry as might depend upon the employment of apparatus of such remarkable magnitude.

"Several scientific gentlemen were present: Professor Renwick, of Columbia College; Professor Draper, of the New York city University; Professor Ellet, of the South Carolina Columbia College; J. R. Peters, C. E., and Doctors Fisher and Gale, assistants to Professor Morse. The arrangements, from their temporary character, were not intended to show the perfect working of the finished telegraph. The powerful battery employed was operated under the disadvantages of imperfect insulation and the dampness of the earth above which they were supported—this was feelingly demonstrated by the liberal supply of shocks given in every direction by the slightest contact. Notwithstanding these disadvantages, the results were such as to afford the utmost gratification to all present.

"The battery employed was of the form known as 'Groves's Constant Battery,' with plates of platinum in pure nitric acid, and amalgamated zinc in dilute sulphuric acid—the two liquids separated by a porous diaphragm. One hundred of these pairs were sufficient to work the magnets through the whole one hundred and fifty-eight miles of wire, and that too in the space of a scarcely appreciable fraction of a second of time. A portion of this interval was consumed in overcoming the resistance of the spring attached to the moving point and the friction of the joint, so that, as far as speed of communication is concerned, we may safely say that the action is instantaneous through this length of wire. This result, although in accordance with the known laws of electric action, was yet gratifying as affording a confirmation of them when applied to an 'extreme case.' The use of a larger number of pairs of course increased the power of the magnets.

"A series of experiments was then made to ascertain the resistance to the passage of the electric current by various lengths of from two to one hundred and fifty-eight miles of wire. The result was again in accordance with what had been predicted. This resistance increases rapidly with the first few miles, and less and less rapidly afterwards, until for very great lengths no sensible difference can be observed. This is a most fortunate circumstance in the employment of electro-magnetism for telegraphic purposes, since, contrary to all other modes of communicating intelligence, the difficulty to be overcome decreases in proportion to the distance.

"Several other experiments, suggested by the unusual opportunity of a very large battery, were then made; and one circumstance throughout the whole time consumed, several hours, was worthy of note—we refer to the remarkably constant effect of the battery. This is one of the advantages of recent improvements, since the apparatus formerly in use was subject to a very rapid and permanent loss of power.

“Since the experiments were made we have seen in the Glasgow ‘Practical Mechanic and Civil Engineers’ Magazine’ a description of the telegraph of Messrs. Cooke and Wheatstone, a more recent invention than that of Professor Morse, as may be seen by reference to the report of the committee in Congress on the bill giving an appropriation for a trial of Professor Morse’s plan. Notwithstanding the early date of this invention, (1832,) the article in question gravely asserts that the history of electro-magnetic telegraphs dates from the year 1836, although the priority of invention by Professor Morse was known to most of the scientific men of Europe.

“An attentive examination of the two plans will soon convince any one understanding any thing about such matters that they never can become rivals. The telegraph of Cooke and Wheatstone, although similar to that of Morse in principle, is totally different in its applications. In the English telegraph a number of signs to denote the letters of the alphabet, or the signals of any arbitrary code, are *exhibited*, not written down. They may be compared to the manual alphabet of the deaf and dumb, with this exception, that three successive signs are required for some letters, and the dial must be attentively watched or the signals are lost and cannot be recovered unless by a second transmission. In the American telegraph the intelligence is written down, and any thing expressed by ordinary written language, letters, figures, or ciphers may be instantaneously transmitted and recorded, even in duplicate, triplicate, or quadruplicate, if desirable. The absence of an attendant, therefore, makes no difference in the reception of intelligence. The American invention has the advantage also in point of expense, and, from several ingenious improvements in the mode of preparing and laying the wires, we feel assured that its liability to derangement is far less than that of the more costly English telegraph.

“In its most elementary form, the apparatus of Cooke and Wheatstone has been in use upon several railroads, and if the power of transmitting but two signals, as on the Blackwell railway, is worth the original outlay, the possession of an unlimited communication for the purposes of the company or of individuals must certainly be worth much more, and yet the cost is in fact less.

“A single tract of railroad of any length can be made as effective and as safe by means of this auxiliary as any double tract can be, and this, too, at an original outlay of about the sum required annually to keep a tract in repair. The advantages to railroads of this important invention can easily be understood by those familiar with railroad management; and if to these we add the profit to be derived from the transmission of intelligence, we certainly think there is ample inducement for its employment upon every railroad in the United States.”—*National Intelligencer, Washington, Oct. 18.*

Manchester Institute of Natural and Experimental Science.

ON Wednesday evening, the first conversazione for the present session was held at this institution; the subject being a paper by Mr. W. Sturgeon, "On some Peculiarities in Magnetic action of Ferruginous Bodies," with experimental illustrations. Magnetism, observed Mr. Sturgeon, having now become so interesting a subject, every fact connected with it, however remotely it might appear to be situated from direct practical application, must always claim a specific value within itself, which at some future day might be turned to account, and become highly important. Some of the facts he was about to describe might be classed amongst those new-born magnetic phenomena that appeared, at first sight, little more than novel curiosities, but which might eventually become of more importance than at present we were capable of predicting. There were, however, some other facts which might be taken immediate advantage of, in consequence of their direct bearing on one of the most important applications of extensive magnetic forces that had hitherto been attempted by man,—that of converting those forces into motive power. The first class of facts which he had to offer to the notice of the audience, consists in the polarisation of soft iron, by its vicinity to a permanent magnet, and its action on a compass needle whilst under that influence. These facts showed, that, when pieces of soft iron were placed between the pole of a magnet and a distant compass needle, at different distances, there were two positions in which the iron could be placed which produced maximum deflections. One of these was when the iron was close to the magnet, and the other when it was close to the needle. Between these two positions there was another, where the iron gave a minimum deflection. This latter position referred to the centre of the iron; and, what was very curious, this point was that which referred to the centre of every piece of iron, whatever might be its length. Mr. Sturgeon then produced six pieces of iron, varying from one inch to six inches in length; and each piece produced its minimum deflection when its centre was placed at the exact point between the magnet and needle, at which all the others were found to give their minimum deflections. Although he had, in the introduction, considered this class of facts as mere curiosities at present, he thought he saw his way in applying them to some useful purpose. He merely threw it out as a hint; but he thought they might be applied to correct the very extensive local action which was known to affect the compass on board of ships. The next class of facts related to the magnetic action exerted by a bent bar of soft iron, when excited by electric currents from galvanic batteries, of different numbers of pairs of the same kind, and those pairs differently arranged; the iron on which the action was exerted being in close contact with the poles of the excited bar, or electro-magnet. The following experiments were made with a large magnet at the Victoria Gallery, with

the view of gaining information as to producing motive power from an electro-magnet. The magnet was a soft iron horse-shoe magnet, enclosed in 14 spirals of copper wire, seven on each branch, the wire of each coil 70 feet long. The cross-piece, as it was called,—the piece of iron which crossed the poles of the magnet, as in all these experiments,—was in close contact with it.

Experiment.	Battery.	Weight carried.	Power overcome.
First....	19, Daniell's, in series.....	8½ cwt.	9 cwt.
Second ..	Do. in two series of 10 cells each.....	10 "	10½ "
Third ...	Do. one cell only.....	7 "	7½ "
Fourth ..	Do. 19 cells as one pair.....	10 "	10½ "
Fifth	Do. 16 pairs in series of 4 pairs each.....	10½ "	11 "
Sixth	Pot battery, single pairs, nitric acid and water..	8½ "	9 "
Seventh ..	No battery connexion.....	2 "	2½ "

In addition to the above weights, the magnets had also to sustain the cross-piece, scale-board, &c. amounting to 1¼ cwt. Another class of facts were obtained from the same electro-magnet, when excited by an iron zinc battery of eight pairs, in a series of four; the difference of the cross-piece from the poles being different in each case.

Close contact.....	13½ cwt.
Two cards intervening.....	4 "
Four do.....	1 "
Six do.....	cross-piece only.

Several gentlemen present were aware that a very beautiful electro-magnet was made by Mr. Joseph Radford. Mr. Radford's magnet would not hold the cross-piece at the distance of one card thickness. By comparing the facts afforded by these two magnets, we were led to understand that the magnetic action on soft iron, in close contact, was no criterion of the attractive power at a distance; and that the latter was greater in long than in short electro-magnets. The next class of facts that he should have to notice would develop another important feature in the magnetic arrangements best adapted to command attraction at a distance from the attracting poles.—The experiments here introduced were with two long bar magnets and a distant compass needle, and showed that the deflection was considerably greater when the magnets were a little separated from each other than when close together. The object of these and other experiments was to afford information on some of the most important points connected with the application of magnetic forces to motive power, which, as far as relates to the figure of the magnet and the distances of the poles from each other, seemed to be very conclusive.—*Manchester Guardian, Nov. 8, 1843.*

Abuse of the Patent Laws.—Roberts's Galvanic Battery.—New Diaphragm.

SIR,—A most absurd practice appears to have become common amongst certain individuals—a practice, the continuance of which will offer serious obstacles to the freedom and progress of scientific invention and discovery. With your permission, I wish to call the

attention, and, if possible, procure the assistance of yourself and of your readers, in endeavouring to arrest the progress of an evil of no small magnitude.

I refer to the numberless instances in which parties obtain patents for inventions to which they have not the slightest foundation of a claim—such discoveries having been previously made known for the benefit of the public. I was particularly struck with this fact in reading the account which you gave, a few months since, of a patent taken out by Mr. Woolrich, of Birmingham, for gilding, plating, &c., by means of the magnetic electric machine. The patent was obtained in the latter end of the year 1842. Now, in a work published by Mr. Sturgeon in the early part of the same year, occurs the following passage:—

“It is now more than seven years ago that I contrived a magnetic electric machine, by means of which I coated metals with tin, copper, &c., and I have employed the same machine to advantage in gilding silvering, and platinizing various kinds of metals of inferior value, and I have no doubt that in this capacity the magnetic electric machine may become generally useful, I have produced good electrotypes on a small scale by its employment.”

Here, sir, is the very invention particularly described and practised, for which, some months after, Mr. Woolrich obtains a patent, and I, along with others, who have practised or intend to make use of the plan proposed by Mr. Sturgeon, have the satisfaction of seeing the patentee step in, with “Stop! where is your license?” If Mr. Woolrich had made some ingenious improvement in the magnetic electric machine, and then, from its superior utility, laid a claim for its application to electro-metallic purposes, our surprise might not have been much excited; but when he gives a description of the usual form of that machine, and then claims its appropriation to gilding, plating, &c., as an invention of his own, after Mr. Sturgeon had published it for the benefit of all, it is assuredly a case of self-appropriation which one may be well excused for not submitting to.

It is especially in connexion with the subject of electro-metallurgy that this abuse exists. There is scarcely one of the ingenious inventions and devices introduced by Mr. Spencer and others, which has not been patented by other parties, thus committing, I may say, an act of robbery on the public, and of injustice to the original inventor. Imagine, sir, the surprise of Messrs. Daniell, Grove, and Smee, if a patent were to be obtained next month for the various voltaic arrangements invented by those gentlemen; or how would General Pasley sustain his gravity if a patent were to be granted for exploding gunpowder, or other *analogous* material, by means of a wire ignited by the voltaic battery. Yet such things may be. Mr. Spencer’s avowed object was to obtain a metallic surface for printing, &c., but this part of the invention is now monopolized by patent. The same individual also discovered a method of causing the voltaic deposit to adhere by means of nitric acid, yet another

person has the "coolness" and folly to claim the device as his own, by procuring a patent for it. Mr. Charles O. Walker discovered a plan of plating and gilding with silver and gold anodes, thus effecting a great reduction in the expense of these processes, and almost immediately after a patent, of course, is granted to another individual for the use of those discoveries. More than one patent has been obtained for modifications of the voltaic battery, whilst the science of photography is enveloped on every side by these protecting influences. Endeavour to conceive to yourself, Mr. Editor, the learned philosopher, Volta, securing the right to use the invention which bears his name, by obtaining a patent in every country in the world for the voltaic trough; or suppose that every lightning conductor were to be dubbed with "Benjamin Franklin, patentee." At the meeting of the British Association, Mr. Fox Talbot gave to the public, as I had ignorantly supposed, a plan of multiplying spectra by the electroscope, stating, at the same time, that two other individuals had practised a similar invention; but some months after, the above plan, so generously given *pro bono publico*, is returned to its lawful owner by means of a patent. Truly there is something comprehensive and universal about these patents which ought to excite our admiration and wonder. I would sincerely recommend Mr. Thomas Spencer to obtain a patent (and who shall hinder him?) for making cupreous deposits for any and every purpose whatever, by means of a current of electricity, steam, or other *analogous* fluid, and to improve his undoubtedly original invention by including amongst his deposits every metal under the sun, save and excepting *brass*, the monopoly of which he may safely and profitably leave to his successors in the electric art, or "cold metallic foundry," as Jacobi has it.

No doubt you will have seen the account of the grand explosions near Dover, by the use of the voltaic trough, and as you have previously recommended for that purpose the form of battery proposed by Mr. Roberts, I wish to take this opportunity to caution your readers from using an arrangement which, in my opinion, is injudicious. I am led the more to allude to the subject, because, probably, like many others, I relied on his supposed experience, and, without much reflection on my own part, constructed a rather extensive battery on the plan proposed, and was considerably mortified with its failure. The error does not exist in the materials employed, which are zinc and iron, which elements promise to be of great utility, though I am inclined to believe they were first proposed by Mr. Sturgeon; but the fault lies in the arrangement of the two metals. On reference to the plan, as depicted in your last year's Magazine, it will be seen that each zinc plate is connected with an iron plate; only by a peculiarity in the arrangement, there is interposed between every zinc and iron plate so connected, two other plates of the same series. It is well known that when a zinc and iron, or other similar plate, are connected together, and then immersed in dilute acid, the zinc plate is rapidly dissolved,

a current of the positive galvanic fluid passing from the zinc through the liquid to the other plate. Mr. Roberts's idea seems to be that by placing two other plates between the zinc and iron, which are joined together, the galvanic fluid would be arrested without the use of partitions, as usual; but as the interposed plates are good conductors of the galvanic fluid, such an idea is fallacious, and as soon as the battery is immersed in the acid, action commences, and in a short time the zinc plates would be destroyed without having been used at all. It is on this principle that I have made use of a diaphragm for separating the two fluids in Daniell's battery, and in some others, where such diaphragms are required. A multiplicity of substances have been proposed for this purpose, amongst which most commonly used are animal membrane, bladder, brown paper, plaster of Paris, wood, and porous earthenware, the whole of which have some defect or other, which is a source of trouble to the operator. Now I find that a metallic diaphragm surpasses by far every other, preventing, as it must do, the least mixture of the fluids, and, at the same time, allowing the galvanic fluid to circulate without any obstruction at all, a desideratum not obtainable with any other form of diaphragm. When the fluids have no action on copper, I prefer to use it as the substance for the diaphragm; but when nitric acid is used, as in Grove's battery, I employ copper, gilt or platinated on the side of the nitric acid, or else use a cast iron diaphragm, on which material the nitric acid has but little effect. The only precaution requisite in using a metallic diaphragm is to prevent it from coming in contact with the zinc or copper, &c., of the arrangement, in which case, of course, a local circle would be formed, and the general effect would be lost.—I remain, sir, your obedient servant,
(*Mechanics' Mag.*) C. W.

Electro-Magneto Plating.

Sir,—In a letter which appeared in your number of last week, containing an exposure of the common and fraudulent practice of appropriating and securing other persons' inventions by patent, mention is made of a patent, which is conspicuously held up as one of "the numberless instances in which parties obtain patents for inventions to which they have not the slightest foundation of a claim; in reply to which, I beg the favour of the insertion of the following remarks.

The writer asserts that Mr. Sturgeon published an account of my magneto process of plating, &c., in 1842. To this effect, he quotes the following passage:—"It is now more than seven years ago that I contrived a magnetic electric machine, by means of which I coated metals with tin, copper, &c.; and I have employed the same machine to advantage in gilding, silvering, and platinizing various kinds of metals of inferior value, and I have no doubt that in this capacity the magnetic electric machine may become generally useful. I have produced good electrotypes on a small scale by its employment."

As far back as the year 1834 I was engaged with my father in experiments with the magnetic machine and had a large one con-

structed, by which we decomposed water, iodine of potassium, and other compound solutions, and were the first, I believe, to produce a continuous current by the magnet, which was never published beyond the lecture room of the Royal School of Medicine, of this town. Two years afterwards we applied the magnet to the decomposition of copper. In 1839, we used it in depositing silver from the ammoniacal solutions, with a view to plating; but our trials, although partially successful then, were not brought to that perfection requisite for practical purposes, until I accomplished this desideratum in 1841, and, at the same time discovered a new class of solvents. I then applied for a patent, and owing to a patent being unspecified before mine, was advised to wait a few months, *during which time* Mr. Sturgeon published his work, *stating* that he did it, but without explaining and mentioning *how* he effected this object.

In answer to what "C. W." says—"If Mr. Woolrich had made some ingenious improvement in the magneto-electric machine," I would refer him to my specification, and Mr. Sturgeon's account of his machine, a perusal of which will show that there is an essential improvement and difference between the two—the general form of both machines being taken from that of Mr. Saxton's, which was made in 1833. Some idea, I think, will be formed of the improvements which I have introduced, when I state that I can construct a machine for about £15, which is capable of depositing 60 ozs. of silver per week; and hope still to reduce its cost considerably. Mr. Sturgeon's merit as an electrician, and his valuable assistance in the collateral branches of science, entitle him to the regard of every scientific person; but, although, like others, he may have contributed to the advancement of this department of science, he certainly has not applied it, *practically and beneficially, to the arts*. It was for doing this that I obtained my patent.—I am, sir, yours, respectfully,
(*Ibid.*) JOHN STEPHEN WOOLRICH.*

An Improvement in the Submarine Propeller.

THE improvement alluded to is that recently invented and patented by Lieut. Hunter of the United States Navy. It may be briefly described as consisting in substituting for the ordinary vertical paddle-wheels, *horizontal* paddle-wheels placed amidships of the vessel, about half way between the deck and keel, and constantly submerged.

The first boat built on this plan, about two years ago, was called the *Germ*. The following is the description given of her in the *New York Herald* on the occasion of her first experimental trip on the Hudson:—"She is 50 feet in extreme length; width of beam at the water line 9 feet only, at the gunwale 11 feet. The area of her displacement at the greatest breadth of beam is a fraction over 20 square feet. She is propelled easily 8 miles an hour; and with better engines, could easily be propelled 11 miles an hour. She has two engines; each of which, if properly constructed, would be equal to

* See further particulars at page 160 of this volume.—EDIT.

what is called five-horse power; they are, however, so indifferently arranged, as to work at a loss of one-third of what should be their power; and have therefore together but six and two-thirds horse-power. Calculating on this data, it will be seen that the propulsive power used in the *Germ* is equal to one horse for every 3 square feet; whereas the propulsive power used for our fastest steamers is equal to three horses for every square foot of displacement. The contrast, therefore, is very great. The well established fact, that the power necessary to propel a vessel is estimated by the area of her displacement at the greatest breadth of beam, and the advantage of speed known to result from great length of keel, and the application of paddle-wheels of greater diameter, leaves us the interesting and valuable truth clearly self-evident, that the submerged horizontal paddle-wheel is a much more efficient propeller than the paddle-wheels now in use. The great advantages consequent on the use of this new kind of propeller, for ocean navigation, is too evident, therefore, to require much stress. By its lateral action, the movement of the vessel is always under control; she is in no danger from broaching to, or bringing by the lee, as, by the peculiar power of her paddles in the lateral action, she can easily be brought out of the trough of a sea, and be made to "head it," without the aid of a rudder, or without headway on her. And, more than this, the propellers being at all times submerged, the vessel moves through head sea with but little diminution of her speed. The *Germ* has the appearance of a handsome canal boat. No wheels are seen, very little smoke, and a very small escape pipe, are all that tell she is a steam-boat. She moves with great velocity, and perfectly noiseless, with scarcely any rippling of the water. She turns easily, almost upon her own centre. Half the boat is formed into a neat cabin; the forward half is occupied by the engine and boiler, which is on the high-pressure plan. The boiler is made on the locomotive plan, with cylinders attached to it, larboard and starboard. In the cylinder works a small piston-rod, 18 inches long; from this extends the connecting-rod, about 4 feet long, and this last rod is attached directly to the paddle-wheel crank. The crank on each side is connected with a vertical shaft that works the paddles exactly like a man working two coffee-mills with his two hands. The great feature in this boat, that is, the propeller, consists of a hollow iron hub, 4 feet in diameter, with paddles made of boiler iron radiating from it. The superficies of each paddle is one-half of a square foot, therefore the whole diameter of the paddle-wheel is exactly five feet. There are two of these paddle-wheels, the space between them being occupied by the keelson. These paddle wheels cannot be injured by the vessel's grounding, for the bottom of the vessel is always below the paddle-wheels, ground where she may. The *Germ* has an advantage over all vessels otherwise propelled, in not requiring a rudder to direct her course, by reason of the lateral action of her wheels. For she can be perfectly steered by her throttle-valves and starting-bars. She has a rudder, but it is more for convenience than actual use. It will be easily seen, too, that the hull of a steamer

thus constructed must be subject to less wear and tear than all others, for the power of the engine is imparted in a line with the keel, and at a point most available for propelling. Again, the paddle-wheels are not subject to the irregular action of the sea, and therefore they have always a uniform resisting power, and her engines work smoothly. On the other hand, if a vessel thus constructed chose to use sails, her paddle-wheels offer less resistance than those of any steam-boat; take off the connecting rods, and her wheels do not present a resistance of half a knot out of every ten knots. Her paddle-wheels are of iron, made very simple, but strong. They are not liable to get out of order, and although made of boiler iron, are so buoyant as to float, owing to the displacement caused by the hollow hub in the centre."

The success of the *Germ* led to the building of a much larger vessel, called the *Union*, said to be the largest vessel for her beam which is afloat. She is 185 feet long, and only 24 feet beam at the water line. Her tonnage is 1,040, and average draught, 10 feet. She is described as being remarkably sharp at the bows, and her lines as being altogether exceedingly fine. Her speed is variously stated at from seven to fourteen miles an hour; but I see no evidence of her having done more than between eight and nine, and that evidence is not of the best. Indeed, if the following extract from a letter in the *Express* may be considered trustworthy, it may be considered doubtful whether she can accomplish more than six on a fair average.

"A gentleman of high respectability in Alexandria, and withal a man of practical scientific attainments, made two recent observations by way of testing the rapidity of her movement through the water, which I am permitted to make public. The occasion embraced by the gentleman was one most favourable for the *Union*, it being at a time when on an experimental trip, the President and his Cabinet being on board, and, of course, the vessel was put on her best speed. This person stationed himself on the land, and, with an accurate instrument, measured by a second watch, the time required for the length of the vessel, 185 feet, to pass his line, by which he was enabled to measure to a fraction her actual speed. In going down the Potomac she had the wind and tide in her favour, at which time, by this measurement, her speed was little less than seven miles the hour. The wind having changed on her return, she came up the river with the wind, and against the tide, making five miles the hour; from which it is fair to deduce that her actual speed through the water must have been about six miles the hour. This statement of her speed corresponds very nearly with other observations, made, I think, at the same time. The small steamboat *Phoenix*, which runs between Washington and Alexandria, left the latter place, as I am informed by persons whose word I should not dare to doubt, fifteen minutes *after* the *Union* had passed on her way to Washington, and caught up with her in twenty minutes."

The crack United States steam frigate *Missouri* has been repeatedly stated in the American papers to perform over twelve

miles per hour, *through the water*; and the *Empire* and the *South America* more than *twenty*. Making all possible allowance, therefore, for any exaggeration in these accounts, it is evident that so far as mere *speed* is concerned, Lieutenant Hunter's is an improvement in the wrong direction. Neither, I fear, is there much to choose, as regards the matter of safety, between propellers submerged wholly or partially at the stern (such as those of Smith, Blaxland, and others) and Lieutenant Hunter's entirely submerged side propellers.

It appears, nevertheless, that the American government have given orders for constructing several *revenue* cruisers on the plan of the *Union*—revenue cruisers which, if meant to catch smugglers, ought to be the fastest of any! The only match to this that I know of, is afforded by the conduct of our own Government in sending out the heavy sailing and unmanageable *coffin* gun-brigs to chase and capture those miracles of speed, the slavers on the coast of Africa.—I am, sir, your constant reader,

(*Ibid.*)

AN AMERICAN MERCHANT.

The Advantages of Wire Rope over Hempen ones.

SINCE the introduction of wire ropes in the mines of Saxony, experience has proved them to be more advantageous than hempen ropes, both in cheapness and in durability, which has warranted their general introduction into all the mines of Saxony. The price of the hempen rope of 288 threads is, to the price of the wire rope which replaces it, as 1 is to 0·3483; consequently, the wire rope is 65·17 per cent. cheaper than the hempen rope. The price of a hempen rope of 336 threads, the largest used in the mines, is in the proportion of 1 to 0·3405. Hence, the wire ropes are two-thirds cheaper than the hempen ropes which they replace, and, under many circumstances, of greater durability, particularly in moist situations, or where the rope remains for a long time coiled on the drum, when it decays from mildew. Besides these advantages, the wire ropes are one-third the weight of the hempen rope which they replace. Wire rope of twelve wires weighs, per latcher, (equal to two French meters, or 78 6·8 English inches) 3·4309 pounds; hempen rope, of 288 threads, weighs 9·62 pounds, or as 0·3536 is to 1. The wire used is 3·3 milli-metres in diameter, equal to 0·39 inches, and the strength of the wire ropes is in proportion to the number of wires. The wear and tear of wire ropes is less rapid than by hempen ropes; consequently, the hempen ropes, to have an equal durability with wire ropes, must, at the outset, be much stronger. The maximum strength, or breaking strain, of hempen ropes of 288 threads, is expressed in pounds, equal to 19,800 pounds; whereas, the breaking strain of the wire rope of twelve wires, used in the place of the above hempen rope, is, when new, 11,200 pounds. Wire ropes, having a much less elasticity than hempen ropes, must, consequently, be coiled on a drum of greater diameter. The result of the experiments made at Freiberg, is, that the drum for iron wire ropes must never be less than 8 feet in diameter, and the maximum of the working load must be between one-sixth and one-seventh of the

breaking strain. Wire ropes have been in use since 1834 in the mines of the Hartz, and have lately been introduced at the coal mines of the northern counties in England, and on most of the railroads where stationary engines are used.—*Freiberg Mining Journal*.

Important Improvement in the Sugar Manufacture.

A PATENT has just been perfected for a most valuable improvement in the sugar manufacture. It relates to that part of the process which consists in expressing the juice from the canes after they have been cut and gathered. At present this is effected by means of crushing, or rather grinding mills, with horizontal fluted rollers, set in motion either by water, wind, or steam power. Each mill has commonly three rollers; but in some mills recently constructed, or proposed to be constructed, the number has been increased to five and six. To increase the number of rollers is, however, according to the patentees of the improvement of which we now speak, only to aggravate a great evil. They object to rollers altogether; and, we think, with good reason, as producing at best only a partial expression, to the extent of from 12 to 20 per cent. short of the saccharine matter actually contained in the canes,—as breaking up the canes unnecessarily, crosswise, lengthwise, and in all directions, and thereby expressing from the rind certain extraneous matters that have a most discolouring and deteriorative effect on the saccharine product, and are the chief cause of the tedious and expensive series of clarifying processes which the syrup has now to go through. By an ingenious but simple, and unquestionably most efficient adaptation of the well-known hydraulic press, the present patentees do away with the rollers entirely; they get out of the canes all the saccharine juice which can be got out of them (by mechanical means at least, which are the only means available for this purpose on the large practical scale,) and with as little intermixture as may be of other juices: they do not *grind* the canes, which, as far as regards the saccharine product, is wholly unnecessary, but they squeeze and compress them lengthwise, that is, in the direction in which the saccharine juice naturally runs, to the utmost possible extent. They save, in this way, not only the loss of from 12 to 20 per cent. consequent on the use of rollers, but all the expense of the water, wind, or steam power, now required to keep the rollers going, less the wages of a man or two to work the new hydraulic agent. We cannot, perhaps, better conclude than in the forcible words of a large West India Proprietor, who, writing from Jamaica on the subject, observes, “*If any thing can save the West Indies, this will.*”—*Mechanics' Mag.*

Judicial Committee of the Privy Council, July 5, 1843.

PRESENT---Lords Brougham and Campbell---Vice-Chancellor Bruce---Dr. Lushington---Sir Alexander Johnstone---Sir Hyde East---and Sir Edmund Ryan.

SAMUEL WRIGHT's Patent for Encaustic Tiles.

MR. M. D. HILL, with whom was Mr. Webster, stated that this was an application for the prolongation of the term of a patent

granted January 26, 1830, to Samuel Wright, of Shelton, in the Staffordshire Potteries, for "a manufacture of ornamental tiles, bricks, and quarries for floor pavements and other purposes." The tiles which formed the principal object of this patent, were of the description commonly known by the name of *encaustic*, which had been in considerable use in this country during the middle ages, though of the mode in which they were manufactured no account was extant. Numerous remains of pavements made of such tiles were scattered over the country, and were commonly called *Roman pavements*; but they differed from the genuine Roman tessellated, or mosaic pavement, in two essential particulars; *first*, they were formed of burnt clays, while the tesserae of the Roman pavement were of marble; and *second*, the pattern or figure was inlaid in the case of the English tile, while in the Roman pavement each tessera was of one colour, and the pattern was formed by arranging a number of tesserae of different colours in lines, groups, &c. From the various clays of which these tiles were formed contracting unequally in the process of drying and firing, and also probably from the want of mechanical skill in making the moulds, all the existing remains of old English tile pavement, however beautiful and striking in their general effects, showed great irregularity in the details; the tiles, instead of coming into close and even contact with one another, having wide spaces between them filled in with a dark cement, and the same figures, when repeated, being seldom two alike. Mr. Wright, the petitioner, had sought, not only to revive the use of these tiles, but to manufacture them of a description much superior to any which had been handed down to us. He substituted for the common clays which appeared to have been employed by the old English tile-makers, the finest clays used in the manufacture of china and porcelain, to which a density and hardness can be given greater even than that of marble. And he made use of machinery to make his moulds, cut out his patterns, &c., by which he had been enabled to give an exactness and uniformity to his tiles, and to the figures inlaid into them, unattainable by hand. [A box of specimens was handed to their lordships, which they examined with a great deal of interest.] Mr. Wright had experienced great difficulties, however, in the introduction of his invention, and the patent was on the point of expiring before he had been able to reap any advantage from it. He had expended altogether £1190 15s. 7d., but had received only £332 13s. 5d.; leaving him with an actual loss sustained of no less than £858 2s. 2d. Indeed, the manufacture would, under these circumstances, most probably have been dropped altogether, but that Mr. Minton of Stoke, the eminent and wealthy potter, had lately taken the thing into his hands, and was preparing to embark in it on a large scale under an arrangement with Mr. Wright, and in the hope that their lordships would grant a suitable prolongation of the patent. The much-admired flooring of the Temple-Church had been laid down by Mr. Minton; and had attracted so much public attention to the article, that there was now every prospect of its coming into fashion and extensive use. Mr. Barry had recom-

mended that the floors of the new Houses of Parliament should be laid with it, and there was every probability that his recommendation would be adopted.

The learned counsel then proceeded to call evidence in support of the application.

Mr. Dacre, solicitor, proved the publication of the necessary advertisements.

Mr. R. A. Brooman, of the firm of Messrs. Robertson and Co., Patent Agents, proved the authenticity of the copy of the specification produced.

Mr. Richard Prosser, of Birmingham, C. E., described the process of manufacturing the tiles. They were harder, and would stand wear better than the hardest stone. They were not affected by changes of temperature. The cost was 18s. a square yard; originally it had been two guineas; if numerous orders were received it might be reduced to about 12s. Mr. Minton had advised with the witness professionally as to the best means of carrying out the invention, and had acted chiefly under his advice in the matter. He had been a loser to the amount of about £200 by the contract for the Temple-Church, but he did not mind that so as it served to bring the article into notice.

Lord Brougham. He did it, in fact, as a sort of advertisement?

Witness. Yes. He would rather have done it for nothing than not have had the thing to do. He could say from his own knowledge, that Mr. Wright, the patentee, must have been, up to the present time, a great loser by the invention. The sum which he had received from Mr. Minton for royalty did not amount to more than about £80.

Lord Brougham was anxious to know how the interests of Mr. Wright would be secured in the event of their lordships granting a prolongation of the term of the patent?

The answer of Mr. Hill on this point being satisfactory, the Court enquired of the Attorney General, who attended on the part of the Crown, whether he had any objections to make to a prolongation of the patent.

The Attorney General said he had none. He thought the invention a highly ingenious and meritorious one, and well deserving of some further protection.

The Court consulted for a few minutes, when—

Lord Brougham delivered judgment. The Court thought this was a case in which the patent ought to be renewed for the full period allowed by statute, namely, seven years. And this quite as much for the sake of the public as of the patentee. With a little further protection it was probable that this description of flooring would soon come into very extensive use, and it had been proved in evidence that the more extensively it was used, the more cheaply it could be afforded to the public.—*Ibid.*

END OF VOLUME I.



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